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FOSSIL MAN: A PERSONAL CREDO

LOREN C. EISELEY
University of Pennsylvania

"The route we pursue in time," remarks Bergson, "is strewn with the remains of all that we began to be, of all that we might have become." I never read that line, so discreetly reminiscent of a thousand divergent crossroads at which the human foot has hesitantly paused, without a sense of the magnificent richness of life compared with our bare reconstructions of the human story. Too often we clap the broken parietals of skulls together, compute capacities, describe the minutiae of the accompanying matrix, and wonder why our students are something less than enthralled. Too often the subject of fossil man is treated in academic courses as though it could be studied in isolation, without either student or teacher having any but the most passing acquaintance with the broader fields of biology. Partly this is an outgrowth of traditional course matter; partly it is the result of the nebulous and peripheral no man's land which physical anthropology occupies — taught as it is within the division of the social sciences, yet having its roots in the zoological realm. To-day it is evident that only a clear understanding of the whole field of evolution and its guiding principles can be regarded as adequate preparation for the task of comprehending and investigating biological change among men.

Anatomical data are, assuredly, a fundamental part of human paleontology. So are artifacts and associated fauna. I am not contending that they be slighted in our courses. But how often is the student given the opportunity to sense the historical quality of life as opposed to inert matter, the infinity of choices with which it is constantly confronted, the utter openness of the future? How often has he been allowed

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to ponder the two ways in which the problem of drouth was met 300 million years ago, and how one road has led onward through the active mammals to ourselves, while our ancient cousins still lie sleeping in the mud? How often has he been told that he is the marvelous "cheat of entropy" — a cheat, not in the sense of a negation of the Second Law of Thermodynamics, but in the capture, momentarily, of the stream of descending energy and the driving of it into higher levels of organization. That strange vortex, that complicated and transitory spinning in the long fall toward the heat death, is ourselves.

It might come as a refreshing shock to some of our students, who believe firmly that the scroll of the future is fixed, to observe the teetering balance of earth's history through the age of the Paleocene. The passing of the reptiles leaves a hundred uninhabited life zones and a scrambling array of newly radiating forms. Unheard-of species of giant ground birds threaten, for a moment, to dominate the earthly scene. Primates and rodents contend at slightly different intervals for the new strange seeds and the sleepy burrows in the sun. "In habitat," says one scholar, "many of these early primates may be thought of as the rats of the Paleocene." (Barth, '50, p. 147.)

Through several million years of Paleocene time the Primate order, instead of being confined to trees, experiments with the same grassland burrowing life that the rodents later perfected. The shabby pseudo-rat we have seen ruffling his coat to dry after the night damps of the reptile age finally ascends once more into the twilight of the rain forest. It will be a long time before we see him on the grass again, but he is trying to make up his mind. For good or ill there will be one more chance, but that chance is fifty million years away; somewhere outside of time man is still a part of that absolute infinite of all unrealized possibilities.

This, properly presented to the student, is the story of that wild night-time landscape over which we have traveled in historyless simplicity. We stand before him, the strange fossil locked in stone upon our hands, while there arise about us the suddenly comprehended voices of chaos, the things from which we have held ourselves disdainfully aloof. Now they talk to us, the long ignored whose blood we have forgotten — the gurgling, inarticulate voices out of swamps and tidepools, the choked gutterals of speechless apes who claim us as their sons. These voices can be heard in the classroom — but not in tables of molar width alone, nor in the seriated clatter of computing machines. They become vocal only out of some deep, accumulated wisdom and penetration, the residue within us of "all that we began to be." It is the emotional increment beyond abstract learning that distinguishes the teacher from the pedagogue.

Of the importance of a broad grasp of ecological principles and their application to human evolutionary problems, nothing is more illustrative than the rise of the angiosperms. Darwin himself referred to them as an "abominable mystery," and their rapid, almost explosive dispersion during Cretaceous times was, it is now apparent, a necessary preliminary to much that occurred in the way of mammalian, bird and insect adaptation during the Tertiary period. It is quite evident, furthermore, that except for the wide extension of the nutritious grasses, particularly from Miocene times onwards, man, as we know him, would very likely never have come into existence.

Edward Berry ('20) has pointed out the remarkable contemporaneity between the evolution of seedbearing plants and the warmblooded birds and mammals with their intense energy expenditure and high metabolic rate.[1] The wide extension of the grasslands during the middle Tertiary was a profound stimulus to the grazing type of life and to all animal forms even indirectly dependent upon grazing.

As our knowledge of the grassland apes, the Australopithecinae, increases, it becomes more and more evident that knowledge of the antiquity and character of the East African plains may be of tremendous importance in determining the first possible time of emergence of the protohuman ancestor. Moreover, it is quite evident that we know little of the attractions which may have first tempted an arboreal ape to forsake the shelter of the trees and pursue a cursorial existence upon open prairie. Omnivorous and predatory he doubtless was in his later stages, but what was he when the crossing was made? Was he small — a light, grasshopper-chasing ape with a neurological trend in the direction of an upright posture which the necessity of seeing over tall grass may have stimulated?

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[1] There is a faint anticipation of this view in a letter of Darwin's to the Marquis de Saporta, December 24, 1877. Circumstances do not permit us to quite determine whether the Marquis or Darwin first glimpsed the possibility, but such evidence as exists favors the Marquis.

The recent doubts cast upon the theory of a true brachiating stage in man (Straus, '49; Le Gros Clark and Leakey, '51, p. 115) and the suggestion that the human precursor may have arisen from such a small, light ape of quadrupedal, but active and versatile manual habits only emphasizes once more the necessity of trying to understand the environment which encouraged a four-footed creature to essay an upright mode of progression. If he was not a brachiator, then so much more the marvel as to what environmental factors stimulated this remarkable departure into bipedalism.

A far more extensive ecological analysis of man's early environment, both in terms of its enemies and its advantages is needed before we can gain a clear picture of the life of that old-fashioned mammal who descended upon the grass, but avoided the trap of cursorial and grazing specializations that have terminated the evolutionary history of so many of his brother mammals. Nevertheless, he would not have been there, and we correspondingly would not be here, save for that remarkable factor of the grass. How he survived upon it while eluding its more insistent and obvious demands for conformity is one of the mysteries we have yet to unravel. It will not be solved without a careful survey of the type of environmental niche this anthropoid was occupying while he was yet cultureless. There is no problem in the study of fossil man which underlines more sharply the necessity for correlated studies from a variety of approaches in both the botanical and zoological realms.

Not least among the subjects with which the instructor should concern himself is the history of thought in his own field. Most students tend automatically to assume that the past is dead and that only present text books containing "up-to-date" material need be consulted. If undirected, the student is apt to develop an unconscious arrogance toward the ideas of previous centuries and a somewhat unwarranted faith in those of his own day. Without instruction and illustration he will not grasp the subtle quality of the *Zeitgeist,* the "atmosphere of opinion," which may direct, intangibly, the thinking of our period as of any other. Consider, for example, the fact that in the fifty years immediately preceding Darwin's *Origin* no biologist in England presumed to speak openly in defense of evolution.[2] Only a literary man, Robert Chambers, and he anonymously, dared to bring the subject into the open. Yet when he did so, he found an enormous, eager audience, and there is no doubt that his work, unmentioned in many histories of science, made the task of Darwin easier.

Examples of this sort, the study of such works as Lovejoy's *Great Chain of Being* teach the lesson that even science is not a straight march to the best of all possible worlds, but that, instead, science is a mélange of habit and new discovery, the lingering of the old, mingling with the not fully tested new which, in itself, may even unwittingly reflect old ideas. Under such guidance the student begins to appreciate the fact that the history of scientific error is sometimes as instructive as the history of truth, or he may learn, in the curious and ironic by-paths of history, that "error" may become truth. The critical faculties become sharpened as a consequence, one's own period is seen in a far clearer perspective, and the student is in a better position to weigh with sensitivity and perception the scientific doctrines of his time, and to discount its emotional preconceptions.

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[2] It must be remembered that Erasmus Darwin's published expression of his views (1794) falls slightly prior to the fifty year interval here indicated.

We need, in other words, not alone the stimulus of those ecological studies which may tell us of adaptations which have led man on to occupy the position which he does to-day, but we need also a greater willingness to consider the philosophical meaning of life, the organism, and those great basic questions bound up in ontogeny and phylogeny which American scholars have shown, compared with their European colleagues, an almost puritanical aversion to discussing.

Strangely enough, it is the student whose questions naively phrased, sometimes pierce, unwittingly, through our careful array of measurement and method, to ask us those old, unsettling questions about the nature of life that have troubled man since the beginning. It does not grace us to smile and fend off these questions. They are basic questions and great minds have tried to deal with them. Biological philosophy deserves a place in the human story and, rightly handled, it can fascinate rather than repel the student. Too often it is confused in the mind of the mediocre instructor with ''religion,'' ''dogma,'' ''vitalism,'' or various other assorted bugaboos, most of which are prejudices inherited from a time of religious and scientific quarrels now better forgotten. To instruct the public successfully does not mean that one has to confine oneself to minutiae, or play safe by belaboring the obvious and the dull. Anthropology, by its sprawling and eclectic nature, offers intellectual adventure to its students. They should begin early to deal fearlessly with the nature of life. If they cannot face its complexities at the beginning they will never learn to do so in the years of their maturity.

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NOTE ON THE SKULL OF PROCONSUL AFRICANUS

J. T. ROBINSON
Transvaal Museum, Pretoria, South Africa

TWO FIGURES

The fragmentary skull of *Proconsul africanus,* discovered in October 1948 by Mrs. Leakey, is of great importance because it gives some idea of the appearance of the skull of a higher primate of the Lower Miocene. Unfortunately this specimen has suffered considerable post mortem deformation, and hence there must be some doubt regarding the true shape. On account of the uniqueness of the specimen it is important to arrive as closely as may be at this true shape. Graphic reconstructions have been given by Le Gros Clark ('50, '51) along with descriptions and discussions on phylogeny.

Through the kind offices of Mr. W. N. Edwards and Dr. A. T. Hopwood, of the British Museum (Nat. Hist.), and Professor W. E. Le Gros Clark, of Oxford University, the writer was recently able, while in the United Kingdom, to study the original of the specimen. This study led to slightly different conclusions from those already published.

This specimen is considerably crushed and no part can be considered free from distortion. Pressure on the skull has obviously been considerable and acted while it was completely enclosed in a supporting medium, in this case clay. The force which produced the distortion may conveniently be resolved into three components: (a) acting from front to back, (b) acting from right to left (of the skull) and (c) acting vertically downward — assuming the skull to be oriented on the Frankfort horizontal. From the appearance of the specimen it seems probable that this distortion has been produced not only by pressure of the overlying material but also by a certain amount of lateral movement, probably due to local creep in the clay bed. Under these circumstances and in a skull as lightly constructed as this one, one could expect not only appreciable fracturing but also perhaps a certain amount of plastic deformation. Extensive fracturing has certainly occurred — the occiput and skull base have been so badly fragmented that reconstitution of these regions was not possible though much of the bone is preserved. Extensive crackling has allowed considerable distortion without much

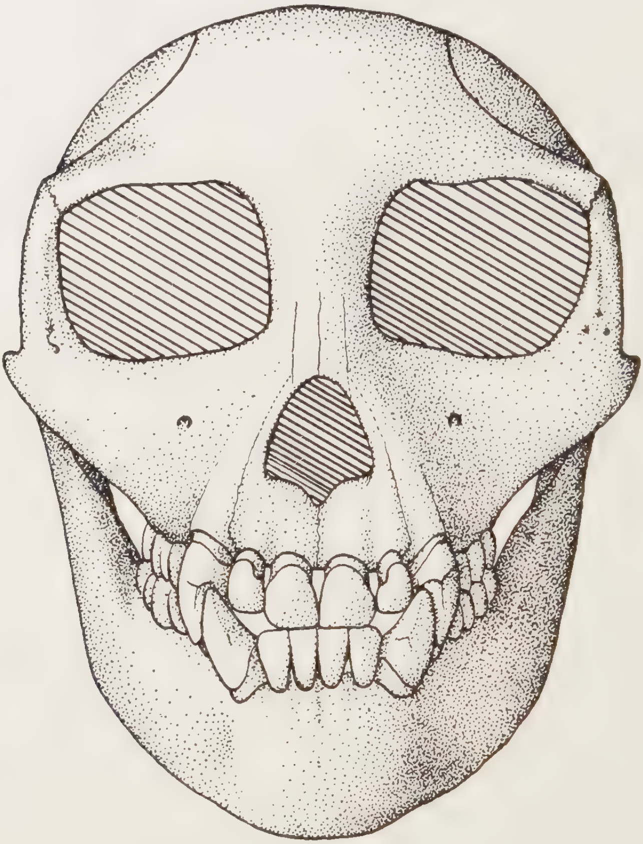

Fig. 1 Graphic reconstruction of the facial aspect of the skull of *Proconsul africanus.* Natural size.

disturbance of surface continuity but plastic deformation without crackling seems also to have taken place. Graphic reconstruction is therefore a complex problem in the absence of other, less distorted, specimens. Some effects of distortion are easily detected. For example, the marked depression in the region of the nasion is not normal and must have been produced by the presence, under pressure, of a small stone or similar hard object in this region. What remains of the skull vault has a number of lines of fracture and discontinuity of surface curvature which show that flattening and upward tilting of the posterior region relative to the anterior

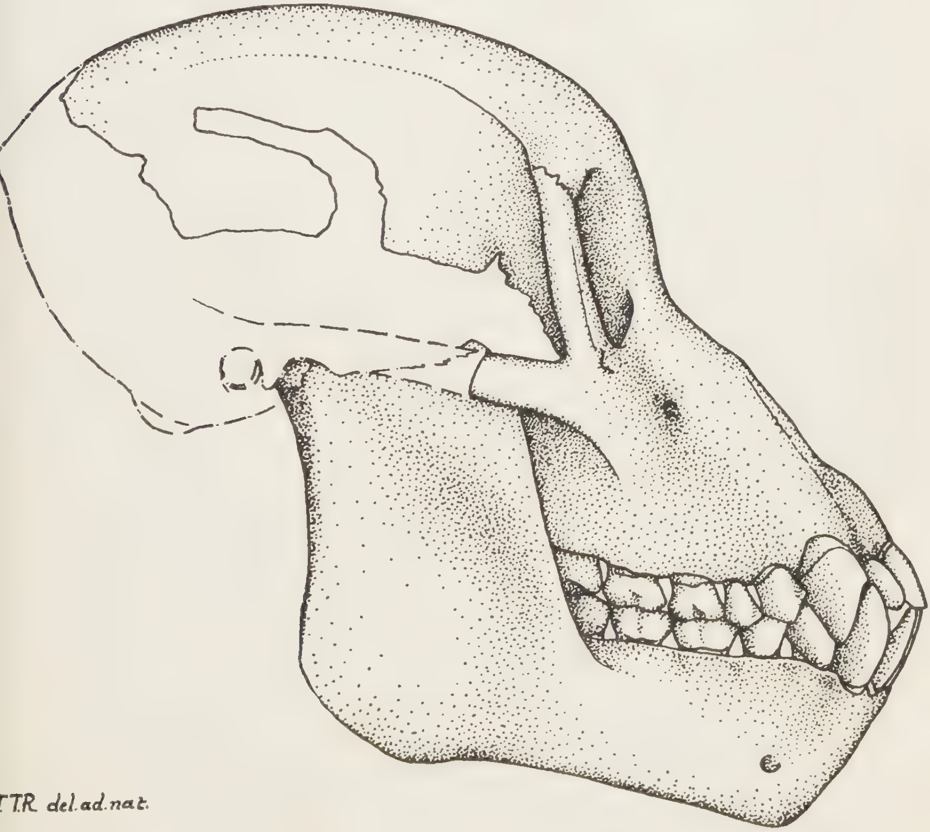
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Fig. 2 Graphic reconstruction of the lateral aspect of the skull of *Proconsul africanus*. Natural size.

has occurred. The crushing backward of the upper portion of the face has exaggerated the above distortion and sloped the lateral orbital margins further backward than was the case in life. The face as a whole has been crushed downward, backward and leftward. If the mandible is placed in position it is manifest that the posterior portion of the palate and dental arcade has been crushed upward in the direction of the orbits. The mandible has suffered least actual damage but exhibits the same type of deformation as does the skull. The lower incisors have been crushed to the left more markedly than have the other teeth.

It will be clear from the illustrations given here that, in the opinion of the writer, *P. africanus* could not have had a high cranial vault or a relatively large endocranial volume.

Le Gros Clark discussed ('51) the systematic position of *Proconsul* and there can be little doubt that he is correct in considering it a true pongid. Doubts on this score have been expressed in various quarters and attention drawn to points of limb structure, relatively long and narrow third lower molar, the shape of the nasal aperture, etc., as supporting a cercopithecoid status. That an early pongid should show characters not found in the recent forms must surely be expected. The further back one goes in pongid phylogeny the less well defined must the ''pongidness'' become. In the opinion of the writer, the dental morphology is such that this form could not justifiably be placed in any group other than the Pongidae. The higher primates exhibit, as far as they are known, considerable conservatism in premolar and molar pattern. The only alternative group into which this genus could conceivably be fitted is that of the Old World monkeys. But, as Le Gros Clark has shown ('51), the cercopithecoid dental type was already established in *Mesopithecus*, a contemporary of *Proconsul*. No signs of bilophodonty are apparent in *Proconsul*, which would mean that a second cercopithecoid stock must have arisen independently, from *Proconsul*, later in the Miocene. However, the more advanced characters of this form fit very well with those of the dryopithecine group of pongids: the "cercopithcoid" characters are ones which may readily be interpreted as being more generalized features inherited from ancestral forms. For example, elongate third lower molars are common among fossil early Tertiary prosimians. It is well known that different parts of an organism evolve at different rates. The simplest hypothesis which fits the present facts is that *Proconsul* is a relatively early pongid in which some characters had not yet reached the condition common among the more recent Pongidae.

The uniformity and conservatism of the pongid dental type has many implications. It is generally believed that hominids arose from pongids and there has been a tendency to regard the *Proconsul* group as an example of the type of pongid which could have been ancestral to both the hominid group and later pongids. Hominids have a complex of dental characters distinct from that of fossil and modern pongids; the difference is as clear in the case of the earliest known pongids as it is in the recent ones. The cercopithecoids and ceboids also each have a characteristic complex of dental characters. More detailed discussion has been presented elsewhere but the safest solution at the present time seems to be that the "family tree" of man is not a tree at all, but a bush. In other words, that the mass of early Tertiary prosimians radiated in a variety of directions in the Eocene and Oligocene, giving rise to a series of independent groups which have remained independent since their inception. These groups — ignoring the prosimians which have continued to the present — are the New World monkeys, the Old World monkeys, the Pongidae and the Hominidae. If this is so *Proconsul* could be ancestral to nothing other than further pongids.

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Hylobatine Sequence of Evolution.— The anatomical characters of *Limnopithecus*, both of the dentition and of the limb bones, appear to be quite consistent with theoretical postulates for representatives of the Hylobatinae at a period so far back as the early Miocene. It is generally agreed that the hylobatine sequence of evolution was initially derived from a generalized catarrhine stock which also gave rise to the cercopithecoid sequence. One would expect therefore that the earliest representatives of the hylobatine sequence would present primitive limb characters corresponding to those still retained by the modern catarrhine monkeys, and that in the course of later adaptation to brachiating habits they would become highly specialized. There is also evidence which indicates that the cercopithecoid and hominoid sequences began to separate probably during the Oligocene period, and that by the early Miocene they had become clearly segregated. However, the early Miocene genus *Limnopithecus* still retained a number of primitive dental features not found in the modern Hylobatinae, and it now appears that the limb skeleton was also more primitive than that of the Recent species.— W. E. LeGros Clark and D. P. Thomas. Fossil Mammals of Africa. 3: Associated jaws and limb bones of *Limnopithecus macinnesi.* Brit. Mus. (Nat. Hist.), 1951, 27 pp., 6 pl.

ON THE IDENTIFICATION OF THE HYPOPHYSIAL FOSSA OF SOLO MAN

S. L. WASHBURN AND F. CLARK HOWELL[1]
University of Chicago

TWO FIGURES

The sphenoid bone of Solo man has been described as being peculiar, resembling that of neither recent man nor apes. The unique conformation includes: a very large hypophysial fossa, a floor of the fossa only 2 mm thick, a short clivus (Weidenreich, '51). These relations are shown in figure 1. Although Weidenreich recognized the danger of estimating the size of the hypophysis (pituitary) from the size of the fossa, he concluded that the difference between Solo and modern man was so great that "the larger size of the sella of Solo man may be attributed to the size of the pituitary gland." Weidenreich ('47) had previously suggested that the condition seen in Solo man was characteristic of the "archanthropines" (Solo, Java, Peking, Meganthropus, and Gigantopithecus), and might be related to their massive cranial skeletons. There are numerous anatomical difficulties with this interpertation, the most probable explanation of the morphology being that the sella turcica is entirely broken away, and that the structure identified as the hypophysial fossa is really the floor of the sphenoid sinus. However, before proceeding to a consideration of this alternative explanation, the deficiencies of the primary evidence should be considered.

As Weidenreich makes clear in both papers cited, the two Solo skulls in which this region of the base is preserved, num-

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[1] We wish to thank the Wenner-Gren Foundation for Anthropological Research for financial assistance, and the American Museum of Natural History, particularly Dr. H. E. Anthony, for the loan of the gorilla skull.

bers 6 and 11, were never completely cleaned of matrix. The most perfectly preserved, number 6, was cleaned through the foramen magnum; number 11 was prepared in the same way and also through a hole in the anterior cranial fossa. Since the cleaning could not be completed under these conditions, the critical areas have never been directly studied and interpretations were made largely from rubber casts of the intracranial cavity. Under these conditions, no final conclusions can be made concerning the detailed anatomy of the sella turcica and sphenoid sinus. Either skull 6 or 11 should be opened so that the cleaning can be completed and a definitive description made. In the meantime, it is worth giving an alternative, and more probable, interpretation of the anatomy of this area of the base.

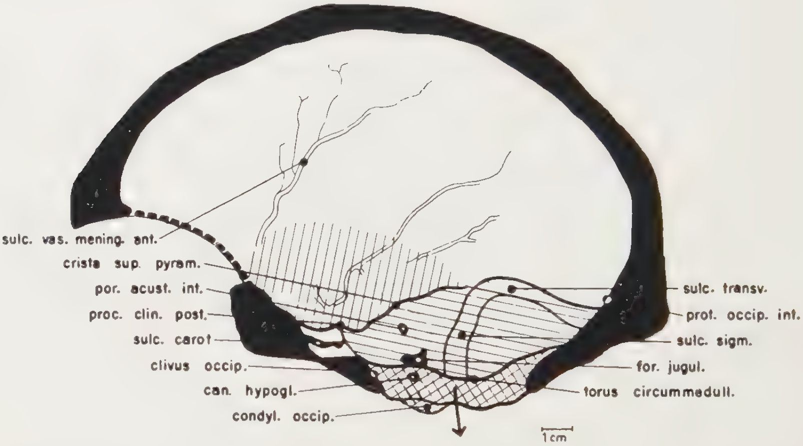

Fig. 1 Mid-sagittal section of Solo number 11, showing large hypophysial fossa, thin base, and short clivus. Weidenreich, '51, plate 46, B.

Plate 1 shows a mid-sagittal section of the skull of a gorilla. It will be seen that the sphenoid sinus is very extensive, the floor is thin, and the clivus, posterior to it, short. If the roof of the sinus in the region of the sella were entirely broken away, the conditions would be very close to those found in Solo skulls 6 and 11. Sinuses of this size occur in modern man. The range of variation is well illustrated in Callander's ('39)

"Surgical Anatomy" (fig. 80 based on Testut and Jacob, '05) and in Toldt ('28, fig. 203–206). Twenty-two of the plates of mid-sagittal sections of skulls published by Ranke (1892) show the sphenoid sinus clearly and in nine of these the sinus extends back of the dorsum sellae. Canuyt and Terracol ('25) call this the large type of sinus and say that it occurs in over 40% of French skulls. As these illustrations show, the sinus may extend well back into the basioccipital bone. In these cases, a condition approximating that of Solo man would be found, if the roof of the sinus were removed. The bone roofing the sinus is very thin, as shown in the plate of the gorilla. This might easily weather away, without any direct force being applied. Actually, both Solo skulls have sustained much more serious damage. In man, according to Callander ('39), Schaeffer ('20), and Pancoast, Pendergrass and Schaeffer ('40), the two sphenoid sinuses are usually of different size, and the septum between them deviates. This deviation is frequently extreme, as shown in the picture of the gorilla. Part of such a deviating bony septum, plus a little adhering matrix, could easily be mistaken for the back of the sinuses and the front of the sella. Probably this is the correct interpretation of the ridge identified as the posterior wall of the sinus in skulls 6 and 11. At least it is clear that all the peculiarities of the sella region as described by Weidenreich, could be interpreted as the normal anatomy of a large sphenoid sinus. Such a sinus would not be rare in either gorilla or modern man. If this is the correct interpretation, nothing is known about the hypophysial fossa of Solo man for it is not there.

A hypothesis which accounts for the apparent peculiarities of the sinus region in Solo man in terms of common anatomical variations, rather than conditions unique among the primates, is probably to be preferred. In certain other fossils, in which the base is known, the anatomy is similar to modern man. This is true for Plesianthropus skull 5 (Broom, Robinson, and Schepers, '50, plate 4), Gibraltar 1 (Sera, '09, and judging from a cast), and Saccopastore 2 (Sergi, '48, tav. 4), judging from the position of the anterior clinoid process. It is also true for the Galilee fragment (Keith, '26). Keith's excellent figure 21 compares the conditions in Galilee, Gibraltar 1, Rhodesian, and an Australian. Both Galilee and the Australian show a condition close to that of Solo 6 and 11; neither Gibraltar 1 nor Rhodesian have been opened so that the condition of the sinus is unknown. Since living primates, including man, and the fossils listed above, all have the hypophysial area in essentially similar form, every effort should be made to interpret the Solo skulls in a comparable way.

In primates, the hypophysial fossa lies anterior to the medial tips of the petrous temporal bones, and the posterior clinoid processes are attached to the petrous bones by a fold of the dura mater. Thus, the dorsum sellae is fixed in its relations to the temporal bones, and the dura, and by the dura to the brain. The dura is connected to cranial nerves three, four, and six, so that a change in the position of the dorsum would change the course of these nerves. Further, the dorsum normally lies just anterior to the pons and the posterior cerebral arteries. If the dorsum should shift posteriorly, there would be no place to accommodate it without changing the structure of the brain stem, the several cranial nerves, and the arrangement of the arteries to the cerebellum and the cerebrum. A shifting back of the dorsum sellae, as far as suggested for Solo skull 11, would thus involve an exceedingly complex series of anatomical changes. The normal position of all the cranial foramina found in the Solo skulls indicates that the usual relations obtained between the bone, brain, dura, cranial nerves, and arteries. The best illustrations we have been able to find, showing the complexity of the relations in this area, are those in Truex and Kellner ('48, figs. 27–28) and Testut and Jacob ('05, p. 219).

The hypophysis develops from the roof of the buccal cavity and from the floor of the diencephalon (Arey, '46; Hamilton, Boyd, and Mossman, '45). This mode of development determines its position relative to the brain, chondrocranium, and ultimately to the sphenoid bone (Arey, '50). Thus for embryological reasons, the hypophysis is very constant in its position.

The nature of its development, as well as the anatomical complexity, make it unlikely that the situation was radically different in Solo man.

The variation of the fossa attributed to Solo man, one dorsum being 28 mm and the other 18 mm from basion, is far more likely to be the variation of a sinus, than of the position of the hypophysis. The distance from the foramen caecum to the foramen magnum is nearly the same in Solo 6 and 11, and, therefore, a posterior displacement of 1 cm in the position of the dorsum sellae would be radically different from anything seen in other primates. On the other hand, if the fossa is really sinus, the sphenoid sinus of man varies greatly in capacity (Schaeffer, '20 and Canuyt and Terracol, '25), and the variation would be perfectly normal.

A simple and consistent way to explain the anatomy of the base of Solo man is that the sella turcica is broken away; what Weidenreich identified as parts of the sella are probably parts of the sphenoid sinus. This explanation accounts for all the apparent peculiarities of the region, without recourse to postulating a giant hypophysis and a unique arrangement of the anatomy of the base. However, all opinions must be tentative until at least one of the skulls is opened and the matrix is completely removed. Until that is done, no one will know if there really are parts of the hypophysial fossa present. Even if a large fossa is present, perhaps indicating a large gland, the relation of the gland to the thick bone would require elucidation. Attention is called to Zuckerman's ('36) trenchant arguments against this kind of uncontrolled theorizing.

Everyone is anxious to extract every possible piece of information from human fossils. Since the bones are none too numerous and often fragmentary, a wide variety of theories is possible. It is difficult to draw the line between justifiable hypothesis and misleading speculation. In the present case, it is unfortunate to suggest that the condition seen in two of the Solo skulls may have been characteristic of numerous ancient men, when the anatomy of Solo is by no means certain. Unfortunately, the general theory that many ancient men had large pituitary glands and that this accounts for their thick cranial bones, is more likely to spread than Weidenreich's own more carefully worded statements. Certainly, the popularized form of Weidenreich's theory of giant ancestors, without qualifying statements or knowledge of the scanty evidence, is something quite different from Weidenreich's ('45) own account, in which the evidence is presented and the extent of extrapolation can be judged.

In all probability, an alternative explanation of the conditions seen in the base, would not have been apparent if Weidenreich had not shown us the original specimens. Our belief, that Weidenreich's description of the base of Solo skulls 6 and 11 is wrong, started with an examination of the actual skulls. Had we only read the reports, the inadequacy of the evidence would not have been clear.

Theories of human evolution can be no more correct than the facts upon which they are based. The best way to make sure of the anatomical facts is for several scientists to study the original specimens. If possible, two or more qualified individuals who hold different theories of human evolution should examine critical specimens at the same time, so that evaluations may be tempered by variety of opinion from the beginning. Stewart ('49) has emphasized the extent to which personal bias affects the understanding of morphology and dating. A clear illustration of this at the present time is given by the various evaluations of the South African man-apes, the "australopithecines." The authors who have studied the original specimens extensively (Broom, Clark, Dart, Robinson) differ from the principal critics (Straus, Zuckerman), who have based their remarks primarily on the literature and casts. One wonders how much of the difference of opinion, which concerns both morphological fact and theory, would be reduced if a group of scientists could settle down with the actual specimens, and discuss the anatomy and its implications.

The Mount Carmel fossils, described by McCown and Keith ('39), have been the subject of extended controversy (Coon, '39; Montagu, '40; Dobzhansky, '44). Much of the debate has centered on whether forms as divergent as Skhūl 5 and Tabūn 1 could be part of a single "neanderthaloid" population or whether it is necessary to postulate hybridization between "Neanderthal" and anatomically modern forms of man. But C. E. Snow has recently made a new reconstruction of Skhūl 5 and it appears that it diverges less from Tabūn 1 than was previously reported. Whatever the facts ultimately prove to be, it is clear that a great deal of effort has been wasted building elaborate theories before checking the facts from the original fossils.

The greatest barrier to the development of a science of human paleontology is the tradition that one person can, or should, make a definitive description of a skull. Such descriptions are too likely to include reconstruction and interpretation which will be regarded as erroneous. The time has come for cooperative research to determine the anatomical facts. After the finder has published a preliminary description, all important fossils should be studied by scientists representing different points of view. Let the description be checked, evaluated, and alternative possibilites published, and technically excellent photographs, casts, and drawings circulated. Before theories are elaborated and debated, the facts should be determined by cooperative scientific effort. Weidenreich may have been correct in his interpretation of the hypophysial fossa of Solo man; perhaps he was wrong. No one will be sure until either skull 6 or 11 is opened.

SUMMARY

It is proable that the hypophysial fossa of Solo man, as identified by Weidenreich, is actually sphenoid sinus. The apparent peculiarities of the region identified as sella turcica can all be interpreted as the normal anatomy of large sphenoid sinuses, separated by a deviating septum.

Comparisons with gorilla, fossil men, and modern men show that similar large sphenoid sinuses are common, and that there is no need to postulate unique structure in the base of Solo man. Gross anatomy, embryology, and variability, all

favor the interpretation that the structures seen are the remnants of the sinus.

A final interpretation of the region must await the opening of Solo 6 and 11.

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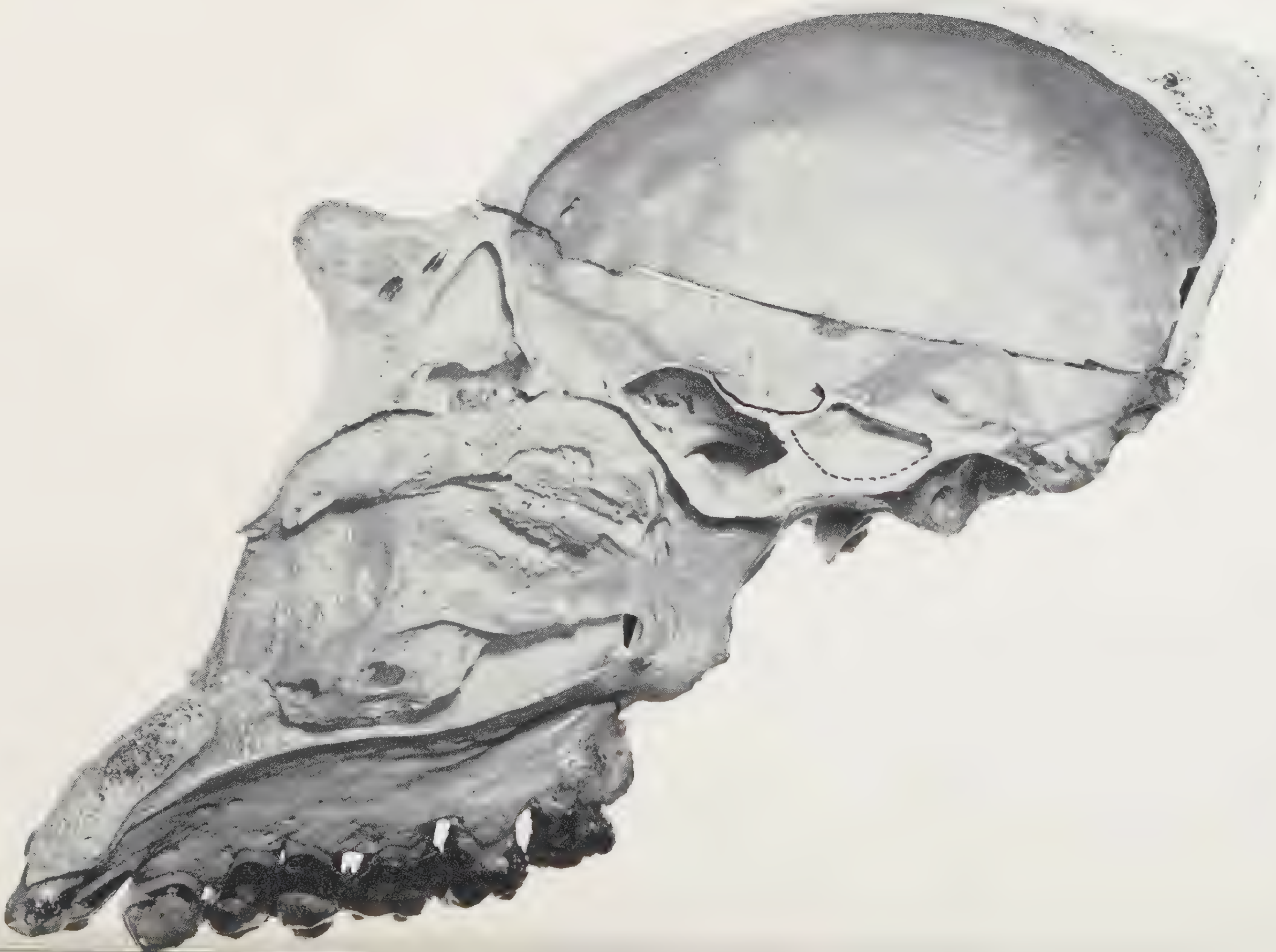

BIRTH ORDER AND BIRTH WEIGHT IN THE CHIMPANZEE

JAMES A. GAVAN
Yerkes Laboratories of Primate Biology, Orange Park, Florida

TWO FIGURES

Howells ('48) in his study of the influence of birth order on body size indicated for physical anthropologists a series of problems, namely the identification of those environmental factors which influence size. He feels that this field of research should be exploited more fully through carefully planned studies and appropriate statistical techniques.

But human beings are subject to numerous influences, both environmental and genetic, which, although they may be identified, cannot always be measured with any degree of accuracy. Some of these difficulties may be obviated by duplicating the human studies on laboratory animals, preferably those closely related to man taxonomically, where more control can be exercised. The primates, although the logical group from which to choose such an animal, have not been used extensively for such purposes mainly because they are difficult and expensive to raise, and because, at least in the higher primates, the life cycle is almost as long as in man.

Fortunately these difficulties are not insurmountable. The Yerkes Laboratories today, some 30 years after their foundation, have a large reproducing colony of chimpanzees. Most of the animals have been born in the laboratories, and their complete life histories are available for study. The staff has accumulated a considerable body of data of interest to the anthropologist, and some of these data are applicable to the specific problem raised by Howells and subsequently extended by Meredith ('50), namely the relation of birth order to body size. The present paper will be limited to an analysis of factors influencing the birth weight of the chimpanzee, and an examination of the biological generalization proposed by Meredith that first-born neonates typically are lighter in weight than subsequent neonates.

The data of the present report consist of 42, live-born chimpanzees, two-thirds of whom were weighed within 24 hours and the rest within 72 hours of birth. For each animal there is the following information on factors which could influence birth weight: length of the gestation period, ordinal of birth, maximum weight achieved by both parents either during their lifetime or until September 1, 1951, and mother's age at the time of conception. From these data the maximum weight of the mid-parent was calculated (average of the maximum weights achieved by both parents). This figure is used as a rough measure of the genetic size potential of the parents.

In the determination of birth order, all live births for each mother were counted. Because in the early years of the laboratories infants were mother-raised, there are often no data on the first born. As a result birth order I has the fewest infants instead of the most as would be expected.

This chimpanzee neonate population differs in two respects from human neonate populations. First, human children of the same mother are in the majority of cases full siblings. For the chimpanzees there were 17 mothers but only 7 fathers resulting in many more half-siblings. Second, although it cannot be assumed that all chimpanzee parents have been subject to the same environmental influences or that such influences have remained constant over the years, still the external environment in the laboratories has been far more constant and similar for the different parents than that of any group of humans.

The average birth weight and gestation period were determined by sex, but as no significant differences were found the sexes were combined. For the total sample the average birth weight was 1.80 kg with a range from 1.13 to 2.37 kg. The average gestation period was 228 days with a range from 204 to 246 days.

The average birth weight by ordinal of birth was determined using Meredith's categories, I, II–IV, and V up. In the chimpanzee the highest birth order was 10. The total correlation coefficients were calculated between the various factors and between each factor and birth weight. Where indicated, the partial correlation coefficient with one or two factors held constant was also calculated. Ordinarily the correlation coefficient is a bidirectional measure of the association between two variables. In the present case a unidirectional relation may be assumed, and the correlation coefficient may be used to construct a diagrammatic scheme showing causal relations. However, the correlation coefficient does not measure quantitatively the contribution of each causal factor.

TABLE 1

Comparison of average birth weight (kg) by ordinal of birth for humans and chimpanzees. The per cent increase (humans) and decrease (chimpanzees) between adjacent categories is also shown. The chimpanzee ordinals extend from I to X

	ORDINAL OF BIRTH				
	I		II–IV		V–(X)
Human birth weight	3.20		3.37		3.47
% increase (Meredith, '50)		+ 5.31		+ 2.96	
Chimpanzee birth weight	1.88		1.83		1.73
N	8		19		15
% decrease		— 2.65		— 5.46	

The average birth weight by ordinal of birth is shown in table 1. The human data (average of Meredith's 18 studies, his table 5, p. 207) show an increase in birth weight with advance in birth order. The greater increase is between the first two categories, I and II–IV. The chimpanzees show the opposite picture: birth weight decreases with advance in ordinal of birth, and the smaller decrease is between the first two categories. There are at least two possible explanations for this difference. The figures may represent a real species difference, or they may indicate that some social factor is operating on the human but not on the chimpanzee data. The first possibility would be very difficult to check as such, but the second may be investigated by determining the relation of various factors to birth weight. If this gives a picture which can reasonably be expected to represent the human situation as well, then an investigation of the social factors operating on human birth weight would be indicated.

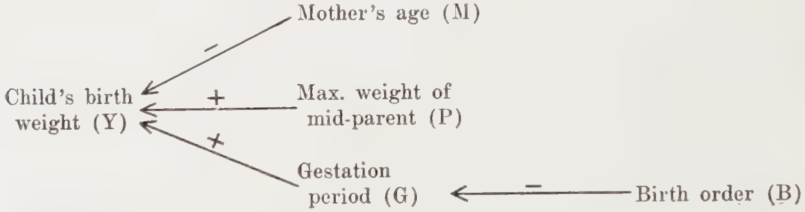

Fig. 1 Diagrammatic representation of the relation of some causative factors to birth weight in the chimpanzee. The sign indicates whether the factor has an increasing or decreating influence.

$r_{GY} = .41^{**}$ $r_{PY} = .29$ $r_{MY} = -.31^{*}$ $r_{BG} = -.33^{*}$

$r_{GY.P} = .49^{**}$ $r_{PY.G} = .40^{**}$ $r_{MY.GPB} = -.53^{**}$ $r_{BY} = -.19$

A consideration of the total and partial correlation coefficients led to the arrangement in figure 1. The reasons for this particular arrangement were as follows:

1. Increase in the gestation period increases birth weight because their total correlation is significant ($r_{GY} = .4081^{**}$),[1] and the significance is retained when weight of mid-parent is constant ($r_{GY.P} = .4874^{**}$).

2. Increase in weight of mid-parent increases birth weight because although their total correlation is not significant it becomes significant when the gestation period is constant ($r_{PY.G} = .4017^{**}$).

[1] The variables involved are denoted as follows: Y, child's birth weight; P, maximum weight of mid-parent; M, mother's age; G, gestation period; B, birth order. Notation for partial correlation coefficients is the usual one, e.g. for "$r_{MY.GPB}$" read "correlation between mother's age and child's birth weight, with gestation period, parental weight and birth order held constant." "*" indicates significance at the 5% level of probability; "**" significance at the 1% level of probability. Statistical methods are those of Fisher ('46); levels of probability were determined from his table V.A. p. 209.

3. Increase in mother's age is associated with a decrease in birth weight because their total correlation is significant ($r_{MY} = -.3133^*$) and when the gestation period, mid-parent's weight, and birth order are constant the significance increases ($r_{MY.GPB} = -.5294^{**}$).

4. Increase in birth order decreases gestation period because their total correlation is significant ($r_{BG} = -.3268^*$) and the significance is not lost by holding the other factors constant.

5. Birth order does not affect birth weight directly because their total correlation is not significant and does not reach significance regardless of what factors are held constant.

6. No relation between either mother's age or weight of mid-parent and gestation period could be found.

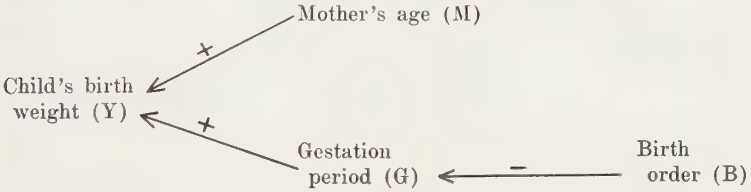

Fig. 2 Diagrammatic representation of the relation of some causative factors to birth weight in human children. Based on data from Goldstein, '38.

$r_{BY} = .15^*$ $r_{BG.M} = -.12^*$ $r_{GY.M} = .18^*$

$r_{BY.MG} = .11$ $r_{MY.BG} = .12^*$

The total and partial correlation coefficients on human children published by Goldstein ('38) were used for figure 2 which was constructed to correspond with figure 1. It can be seen that in the human data when mother's age and length of the gestation period are held constant the original, significant correlation between birth weight and birth order is lost. Also the partial correlation between birth order and length of the gestation period with mother's age constant is negatively significant. The picture for chimpanzees and for humans differs only in the relation of mother's age to birth weight; in the chimpanzee it is negative while in humans it is positive.

This difference may have a biological basis, but there are indications that a sociological factor is operating in the human data. Goldstein was unable to control parent's size while Donald ('39) has shown that familial resemblances account for far more of the total variance in birth weight than any other factor. Also in human societies the lower economic groups, who are heavier than the higher economic groups (O'Brien, '41), have more children and have them earlier than the higher economic groups. Birth order I children would, therefore, be a cross section of the total society while the higher birth orders would become more and more a group selected from the lower economic levels. Also the variability in mother's age will be much greater for birth order I children than for the higher birth orders. If mother's age is of greater importance than birth order, then a decrease in the variability of birth weight for later children would be expected, and was actually found by Goldstein ('38).

It is suggested that future studies of this problem on human beings be so conducted as to consider the influence of parent's size, gestation period and economic position. Until this is done it will be impossible properly to interpret the different relation between mother's age and birth weight in chimpanzees and humans.

Whatever the outcome of such a study, it is apparent that Meredith's biologic generalization, "First-born neonates typically are lighter than the aggregate of neonates of other birth orders" (p. 197) does not get to the heart of the matter. As a statement of observed fact it is certainly true for human neonates. However, it completely overlooks the influence of other, more important factors. The present analysis would indicate that, if all factors except birth order could be held constant, then birth weight would decrease with birth order. Instead of asking the question, what happens to birth weight as birth order increases, it would appear to be more profitable to ask, with what is the variability in birth weight associated.

If anthropologists are to take Howell's suggestions seriously and investigate the multiplicity of factors which influence size, it should be assumed *a priori* that such problems involve multiple causation. The arithmetic mean is a perfectly valid statistic for some purposes, but in problems such as these, where experimental control is impossible and where small samples are the rule, the arithmetic mean is more likely to lead to confusion than to solution.

SUMMARY

The problem is to analyze the influence of various factors on birth weight and to test Meredith's biological generalization that typically first-born neonates are lighter than later ones.

The data were obtained from 42, live-born chimpanzees who were weighed shortly after birth. The following factors were considered: length of gestation period, maximum weight of the mid-parent, age of mother at conception, ordinal of birth.

Correlation coefficients, total and partial, were used to construct a diagram showing the causal relation of the factors to birth weight. The analysis showed that birth weight increases with increase in gestation period and with increase in maximum weight of the mid-parent. It decreases with increase in mother's age. Increase in birth order decreases the gestation period but has no direct influence on birth weight.

A comparison with similar human data gave the same picture with the exception that increase in mother's age was associated with increase in birth weight. It was suggested that this difference could be due, at least in part, to the failure to consider parent size and economic position in the human data.

Meredith's generalization is rejected for, although it is a true statement of fact for human neonates, it does not consider that the problem is essentially one of multiple causation. It is pointed out that in problems such as these the arithmetic mean is not an appropriate statistic.

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* * *

INTELLIGENCE TESTS AND RACE.— Recent investigations by myself and others have forced me to the conclusion that, while intelligence tests are admirable instruments for practical purposes such as educational and occupational selection and guidance within any one cultural group, they cannot be regarded as sufficiently pure measures of innate ability to be employed in comparisons between different groups such as races or nations, nor for genetic studies. And this discouraging conclusion applies to non-verbal, performance, or other tests as much as to verbal Binet or group tests.— Philip E. Vernon. Recent investigations of intelligence and its measurement. Eug. Rev., vol. 43, no. 3, Oct. 1951, pp. 125–137.

SICKLE CELL ANEMIA IN GREECE.— A case of sickle cell anemia in a white adult is described. It is the fourth case to be reported in Greece. Two reports have been published by Dr. Makrycostas of patients treated in Evangelismos Hospital, Athens. Another case has been reported recently (April, 1950) to the Athens Medical Association by D. Kominos, D. Bakalos and P. Katsiroumbas. No case was mentioned in the European literature. Therefore, I conclude that, although this disease is rarely seen in Greece, it does exist in persons in whom no Negro blood is suspected. Bakalos and Katsiroumbas believe that sickle cell anemia may be included in the Mediterranean hemopathic syndromes. . . . Taking into consideration the lack of certain clinical symptoms and its long duration, I conclude that sickle cell anemia is seen in Greece in a mild or degenerated form. — Alexander Zaberdinos. Sickle cell anemia in a white adult. J. Am. Med. Assn., vol. 148, no. 7, Feb. 16, 1952, pp. 549–550.

A COLLABORATIVE GENETICAL SURVEY IN MARSHALL ISLANDERS

R. T. SIMMONS,[1] J. S. GRAYDON,[2] N. M. SEMPLE, JOSEPH B. BIRDSELL,[3]
JOHN D. MILBOURNE[4] AND J. R. LEE[4]
The Commonwealth Serum Laboratories, Melbourne, Australia;
and The University of California at Los Angeles

This paper presents the results of the first of a recent series of genetic investigations made possible by Australian and American collaboration. During the past 10 years numerous blood genetic studies have been made on various populations in the Pacific by members of the research staff of the Commonwealth Serum Laboratories, Melbourne. A feature of this work has been the development of techniques for the collection of blood samples in the field, the transport of samples by air-freight in large "Thermos" flasks to Melbourne, and the testing of all materials under ideal conditions in the laboratory where every facility and multiple testing sera are available. Here every serum used in testing, whether manufactured in the laboratory or collected from a donor, can be checked as to strength and specificity with adequate control cells at the time of use. The advantages of performing this work in a central laboratory over testing samples in the field are well proven.

Birdsell, who has been responsible for arranging American cooperation in areas of the Pacific previously inaccessible to us, has summed up ('50) the objects of these genetical surveys as follows:

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[1] Research Associate, University of California, Los Angeles; Consultant, Commonwealth Serum Laboratories, Melbourne.

[2] Consultant, Commonwealth Serum Laboratories, Melbourne.

[3] Department of Anthropology, University of California at Los Angeles.

[4] With the collaboration of members of the Medical Department, U.S.N., of *U. S. S. Whidbey* (AGS 141).

"The task of the joint program is visualized in terms of identifying those populations critical in providing essential genetical data relative to problems centering on the dynamic processes of human evolution. Various aspects include the identification of the basic racial populations in the Pacific area, evaluation of the degree of hybridization in tested populations, the estimation of rates of gene flow, the exploration of the process of genetic drift, and ultimately the design of experiments to determine the influence of natural selection upon serological antigens."

The results of earlier blood genetic studies referred to above have been published in the Medical Journal of Australia, and the following references to recent papers are primarily intended to bring this Australian work to the notice of American anthropologists, who may not have regular access to Australian journals. Simmons and Graydon ('51) have summarized their results to 1950 for the A-B-O groups, the subgroups of A, and the M-N and Rh types. This paper also presents in tabular form evidence for north-south gradients in the values of blood genes q, m and R^1.

Blood gene frequencies in southern Chinese have been published by Simmons, Graydon, Semple and Green ('50). The results of recent observations on the Lewis blood groups and A, B or H salivary secretion have been published by Simmons, Semple and Graydon ('51), while Simmons and Jakobowicz ('51) have discussed their observations on the Lewis blood antigens and antibodies.

The results of a survey on Maoris have been presented by Simmons, Graydon, Semple and Taylor ('51). This comprehensive survey covers the A-B-O blood groups, subgroups of A, M-N-S types, the Rh types with tests for C^w and D^u, the correlation between the Lewis blood groups and salivary secretion, the agglutinogen P and P.T.C taste tests. This paper further gives the technical methods used for the collection and testing of all samples. Methods for performing slide agglutination tests and the sterile handling of testing reagents have been described in detail by Simmons ('49) and Simmons and Graydon ('50).

It is proposed that papers of this collaborative series be published alternately in the United States of America and in Australia; other papers will continue to be published in the Medical Journal of Australia. Collaborative surveys have also been carried out on the Ainus of Hokkaido, the inhabitants of the Palau Islands, of Truk, and of Kapingamarangi. Other papers at present in press, or in preparation, deal with 5 tribal populations in Borneo, the Gilbertese, the Negritos of the Nassau Mountains, Dutch New Guinea, various groups in the Central Highlands of New Guinea, the Bainings of New Britain, Australian aborigines and others.

The physical anthropology of the Micronesians

(By J. B. BIRDSELL)

Micronesia is an island chain reaching nearly 3,000 miles outward into the otherwise empty Pacific Ocean. It extends from Indonesia in the west to central Polynesia in the east, roughly paralleling the Melanesian archipelago to the south. It is comprised of both high islands, relatively rich in a diversity of food plants, and low-lying coral atolls with a more limited flora. The Marshall Islands are an atoll group lying northeast of the main Micronesian arc, some distance above the main migratory route which passes through the Carolinas to the Gilbert Islands and hence into the core area of the Polynesian triangle. The Marshalls are divided into two clusters, the Ralik group to the west, and the Ratak group to the east. The Marshall Islands supported a high population density. Perthes ('05) states that the 158 square miles of land area supported 15,000 inhabitants, a density of about 95 persons per square mile.

The English language literature contains but two summary articles dealing with the anthropometry of Micronesia: The earlier of these, by Shapiro ('33), reviewed the data available to that time; the later, by Hunt ('50), is more extensive and includes the comprehensive data of Hasebe (over 1,700 individuals of both sexes from all of the major islands of the former Japanese Mandate). The combined results show that the published anthropometry of the Marshall Islanders is of little assistance in determining the racial origin of this population.[5] When Shapiro published, the available anthropometry of the living Marshallese was limited to an early series of 12 individuals measured by Hirako. The small size of this series now renders it useless. Hunt, however, summarizes figures from Hasebe for 236 males from the Marshall group. Of these 165 come from the Ralik group to the west, and 71 from the Ratak group to the east. Comparative measurements are presented in table 1.

TABLE 1

Marshallese Anthropometry

(Males based upon Hunt, '50)

		MARSHALL ISLANDERS		MICRONESIA	POLYNESIA
		Ralik	Ratak	Series ranges	Series ranges
Investigator		*Hasebe*	*Hasebe*	*Hunt* (Compilor)	*Shapiro* (Compilor)
No. of series:		1	1	16	26
Sample size:		165	71	..	..
Stature:	(cm)	162.1	161.6	156.8–164.4	167.5–174.7
Head length:	(mm)	188.4	188.9	185.2–195.6	186.4–199.1
Head breadth:	(mm)	145.2	142.7	139.1–150.1	147.0–160.9
Cephalic index:		77.2	75.6	71.3– 80.9	74.3– 86.5
Face height:	(mm)	127.7	127.0	117.3–132.2	121.5–131.1
Face breadth:	(mm)	138.9	138.2	136.3–145.0	141.3–148.4
Facial index:		92.0	91.9	81.0– 94.5	82.6– 89.9
Nose height:	(mm)	59.1	58.1	54.8– 61.2	52.8– 61.8
Nose breadth:	(mm)	43.8	43.6	40.2– 44.0	40.1– 45.5
Nasal index:		74.4	75.4	68.3– 84.7	69.9– 82.7

These data show but few differences between the Ralik and Ratak groups of Marshall Islanders. The latter seem to be absolutely and relatively narrower-headed, and to have shorter and hence relatively broader noses. As compared with 14 other Micronesian series, the Marshallese are generally intermediate in their metrical and indicial characteristics. Total facial height and nasal height are rather high for Micronesian means. Total facial index is likewise above the average value for this area. Nasal breadth is near the regional maximum, but nasal index is average in value.

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[5] No cranial materials from this island group have been studied.

On a purely descriptive plane, the above metrical and indicial data indicate that the Marshall Islanders are to be affiliated with the remaining populations of Micronesia, and can be distinguished from the taller and generally more massively-built populations of Polynesia. Beyond this crude interpretive level the available measurements provide little information. It is unfortunately true that metrical data alone do not serve adequately to distinguish between populations drawn from different major racial groups. Hence it is hardly to be expected that they can provide suitable instruments for the analysis of the relative contribution of different parental stocks in racially mixed populations.

Since racial taxonomy has been largely based upon the morphological characteristics of living populations, it might be anticipated that such an approach would yield further information concerning the racial origins of the Marshallese. The limited morphological observations of Hasebe, as quoted by Hunt, are barely suggestive. Hasebe found that the internal epicanthus is prominent in Palauans, Chamorros, Marshallese, and among the Polynesian speakers on Kapingamarangi. Among the first two groups this Mongoloid trait is associated with a somewhat higher cephalic index than is usual in Micronesia, but among the latter two, the index does not suggest brachycephalization as a consequence of Mongoloid genetic influence. Hair form is most strongly curled in the western islands and seems to have become straighter among the eastern populations. Thus the Marshallese, despite the somewhat greater nasal breadth in these islands, morphologically seem to show less Negritoid influence than do the inhabitants of the Western Carolina Islands. It may be that with the complete publication of Hasebe's morphological data, it will be possible to more clearly define the racial affiliations of the Marshall Islanders, but at the present time it must be concluded that neither metrical nor morphological data are sufficient to allow an interpretation of the racial origin of these islanders.

Although no satisfactory analysis for the Marshall Islanders is possible at this time, it might be presumed that Micronesia, considered as a whole, should provide more valid materials for interpretation. Dixon ('23), using a type of metrical analysis generally considered unacceptable today, and basing his opinions on two meager series of crania published late in the 19th century, concluded that Micronesia contains a much-mixed population, and properly inferred that the racial history of the region has been complex. He identified various racial types which he believed to have contributed to Micronesia, but a detailed summary would not be profitable. Haddon ('25) was content with the summary statement, "The data for Micronesia are very slight, but these are sufficient to show that it contains a very mixed population." Wagner ('37) working exclusively with cranial materials, merely observed that "The comparative material from the Micronesian Island group is scanty." Hooton ('46) did not specifically deal with the racial affiliations of the Micronesians. If they are subsumed under his Malayan race, as seems probable, he would consider them to be a Mongoloid population which has absorbed older elements from the underlying racial strata of Southeastern Asia. Howells ('44) briefly referred to the Micronesians: "The disappearing population is not well known, but it seems to have been, in race and culture, allied to the Polynesian though with strong Indonesian influences. The language also occupied an intermediate station between the two, all three being related. Little more, unfortunately, can be said about them." Hasebe (Hunt, '50), who has had the most extensive field experience in the region, suggests a sequence of 4 physical types migrating into Micronesia: first there came Negroids, then a small generalized type, followed thirdly by a Polynesian wave of peoples, and finally by Indonesian Mongoloids moving out into Micronesia most importantly influencing the western islands. It is unfortunate that Hasebe's inferences are based, like Dixon's, upon a methodological form of analysis which is unacceptable. One may agree with Hunt ('50) that "The sequence of physical types which entered Micronesia is still obscure."

Even though the classical methods of analysis have failed to reconstruct a convincing account of the migrations into Micronesia, it is still possible, by the crude methods of visual assaying, to designate certain contributory racial components present in the living populations. It is evident, and cultural data are confirmatory, that Mongoloid elements have contributed rather strongly to the present population. There is additional evidence from the Western Carolines that a Negritoid component has been of modest importance in that region. Finally, it must be considered that some form of Caucasoid has become incorporated in the populations of Micronesia. Earlier writers have tended to call this last element Mediterranean in type, but in the more recent writings, such as those of Birdsell ('49) and Avias ('49), this racial component has been considered as Ainoid in origin. In addition to these major racial components, it seems quite clear that another composite group, the Polynesians, must have left some imprint upon this region. Thus to date, no more than the crudest interpretations have been offered in analysis of the Micronesian populations. It seems clear that in this area racial evolution must be unraveled, if indeed it can be, by the techniques of genetical anthropology. The present serological data presented in this paper for the Marshallese represent but a beginning, but it may be confidently stated that when equally comprehensive data have been collected not only in the rest of Micronesia, but for the critical areas surrounding this archipelago, that a more accurate estimate may be made of those racial components which have contributed to the present populations of Micronesia.

Populations sampled in the present survey

It was difficult to estimate the population on any one island or atoll because of the frequent movements of natives from island to island. In many instances the figures given below by members of the crew of the *U. S. S. Whidbey* are considerably lower than those made available earlier to them. The natives themselves gave an even lower estimate of their population.

The estimated populations of various islands referred to in this paper are as follows:

ISLAND	ATOLL	ESTIMATED POPULATION
Kaven	Maloelap	206
Tjan	Maloelap	118
Airik	Maloelap	121
Tabal	Aur	173
Aur	Aur	182
Arno	Arno	256
Ine	Arno	302
Alu	Mili	130
Lukunor	Mili	92
Taka	Ebon	130
Ebon	Ebon	667

Although the tables presented later show samples obtained from 23 natives of Majuro, this atoll was not visited. These natives were, in fact, visitors to other islands where samples were taken.

MATERIALS AND METHODS

The blood and saliva samples were collected between April 25th and June 13th, 1950, at various atolls visited during a survey cruise of the *U. S. S. Whidbey*. After collection the blood samples were stored in a refrigerator on the ship at approximately 5°C., and on the ship's return to Honolulu at the end of June were packed in ice in one gallon "Stanley" insulated jars and air-freighted to Melbourne.

The blood samples which consisted of 4 large drops of blood were added to 1 cm^3 of sterile glucose-citrate blood preserving solution, pH 7.4.

A total of 704 blood samples were collected, and of these only 26 had to be discarded on arrival due to haemolysis. The laboratory investigations extended (as time permitted) over nearly 4 weeks in July owing to the large number of tests (approximately 14,000) which had to be performed. No difficulties with A-B-O, M-N and Rh tests due to the age of the cells were encountered, although some of the samples were 10–12 weeks old when received. When testing aged samples from the field it is our normal procedure to use, as controls, blood samples of known types which have been stored at 5°C. for an equal or greater period in the laboratory. As stated in a number of earlier papers (and more recently confirmed in the U. S. A.) it has been demonstrated by us that suitably preserved blood samples stored at 5°C. and free from haemolysis, give specific agglutination for periods up to, and exceeding 12 months. References to methods of collecting samples and laboratory techniques have been given earlier in the text.

RESULTS

A-B-O groups. Boyd ('39) in his exhaustive compilation of A-B-O blood group frequencies cites only one survey on Marshall Islanders, that of Takasaki who tested 343 individuals. The gene frequencies found were $p = 0.173$, $q = 0.108$ and $r = 0.723$. We are not aware of any blood group investigation on this people published since that date.

In the present survey 678 Marshallese were tested for their blood groups, subgroups, M-N and Rh types. In all the tables except table 5 the natives have been listed according to their island and atoll of origin. The islands represented were Kraven, Tjan, Airik, Tabal, Aur, Arno, Ine, Alu, Lukunor, Taka, Ebon, Majuro and a miscellaneous group of 35 islands. The atolls were Maloelap, Aur, Arno, Mili and Ebon. The A-B-O blood groups and gene frequencies (p, q and r) are shown in table 2.

While there is slight variation in the frequencies of p and q from atoll to atoll, the frequency of r remains fairly uniform. The combined figures for the 678 individuals tested show that

TABLE 2

The A-B-O blood groups and gene frequencies in Marshallese

ISLAND OF ORIGIN AND ATOLL		NUMBER OF BLOOD SAMPLES TESTED	BLOOD GROUPS				FREQUENCY OF GENES		
			O	A_1	B	A_1B	*p*	*q*	*r*
Kaven,	Maloelap	50	38	7	4	1			
Tjan,	Maloelap	35	17	8	7	3			
Airik,	Maloelap	32	14	9	8	1			
		117	69	24	19	5	0.124	0.099	0.768
			59.0%	20.5%	16.2%	4.3%			
Tabal,	Aur	66	31	19	14	2			
Aur,	Aur	59	31	5	22	1			
		125	62	24	36	3	0.125	0.181	0.704
			49.6%	19.2%	28.8%	2.4%			
Arno,	Arno	71	41	13	10	7			
Ine,	Arno	54	26	13	12	3			
		125	67	26	22	10	0.130	0.112	0.732
			53.6%	20.8%	17.6%	8.0%			
Alu,	Mili	30	17	7	4	2			
Lukunor,	Mili	16	12	3	1	0			
		46	29	10	5	2	0.127	0.066	0.794
			63.0%	21.7%	10.9%	4.3%			
Taka,	Ebon	52	26	19	5	2			
Ebon,[1]	Ebon	66	35	11	14	6			
		118	61	30	19	8	0.159	0.104	0.719
			51.7%	25.4%	16.1%	6.8%			
Majuro		23	10	5	7	1			
			43%	22%	30%	4%			
35 other islands		124	56	26	35	7	0.141	0.185	0.672
			45.2%	21.0%	28.2%	5.6%			
Totals		678	354	145	143	36	0.135	0.134	0.723
Per cent			52.2%	21.4%	21.1%	5.3%			

[1] Field workers reported some slight evidence of admixture on Ebon with Japanese and Germans.

354 (52.2%) were of group O, 145 (21.4%) were of group A_1, 143 (21.1%) were of group B, and 36 (5.3%) were of group A_1B. Gene frequencies calculated from these figures were:

$$p = 0.135,\ q = 0.134,\ r = 0.723.$$

A total of 181 individuals of groups A and AB were of subgroups A_1 or A_1B.

The frequency of q is approximately the same as that obtained for Indonesians (0.133), Admiralty Islanders (0.136), Papuans (0.127), Southern Chinese (0.139), Fijians (0.120) and Samoans (0.118).

It was suggested by our field collaborators that a few natives who had originated on Ebon showed some physical evidence of Japanese and German admixture. There is no indication in table 2 that the natives from Ebon were different from those of other islands, and for this reason they have been included in all the tables.

M-N types and S factor. In table 3 are presented the results obtained for the M-N types and gene frequencies tested, $m = 0.222$ and $n = 0.778$. The frequency for m is lower than might have been expected for the geographical location of the Marshalls, and does not fit in with the general trend of the north-south gradient of m for the Pacific referred to by Simmons and Graydon ('51). It may be shown, however, that there are other local directional gradients in addition to a general north-south gradient. Some figures taken from their table show:

Chinese 0.630,
Japanese 0.540,
Indonesians (20 islands, not Javanese) 0.435,
Loyalty Islanders 0.386,
Admiralty Islanders 0.339,
Pine Islanders 0.333,
Fijians 0.332,
New Caledonians 0.297,
Australian aborigines 0.297,
Marshallese 0.222,
Papuans 0.227,
Maoris 0.489,

A total of 305 random blood samples were tested with anti-S serum generously made available to us by Dr. Marjory Pickles of Oxford, England. Of those tested 69 (22.6%) were anti-S positive. In type M 0/22 was positive, in type MN

TABLE 3

The M-N types, the S distribution, and the gene frequencies in Marshallese

ISLAND OF ORIGIN AND ATOLL		NUMBER OF BLOOD SAMPLES TESTED	M-N TYPES			FREQUENCY OF GENES		PROPORTION OF RANDOM SAMPLES S POSITIVE	M-N TYPES PROPORTION S POSITIVE		
			M	MN	N	m	n		M	MN	N
Kaven,	Maloelap.	50	4	9	37			10/40	0/4	0/8	10/28
Tjan,	Maloelap.	35	2	10	23			8/26	0/1	0/6	8/19
Airik,	Maloelap.	32	0	17	15			4/17	0/0	2/10	2/7
		117	6	36	75	0.205	0.795	22/83	0/5	2/24	20/54
			5.1%	30.8%	64.1%			27%			
Tabal,	Aur.	66	3	20	43			2/16	0/1	1/3	1/12
Aur,	Aur.	59	2	16	41			16/39	0/1	4/10	12/28
		125	5	36	84	0.184	0.816	18/55	0/2	5/13	13/40
			4.0%	28.8%	67.2%			33%			
Arno,	Arno.	71	2	23	46			5/20	0/0	3/7	2/13
Ine.,	Arno.	54	2	23	29			2/17	0/2	1/7	1/8
		125	4	46	75	0.216	0.784	7/37	0/2	4/14	3/21
			3.2%	36.8%	60.0%			19%			
Alu,	Mili.	30	4	8	18			1/22	0/3	1/8	0/11
Lukunor,	Mili.	16	1	5	10			4/12	0/1	1/5	3/6
		46	5	13	28	0.25	0.75	5/34	0/4	2/13	3/17
			11%	28%	61%			15%			
Taka,	Ebon.	52	2	15	35			1/22	0/2	0/7	1/13
Ebon,	Ebon.	66	4	25	37			5/18	0/0	2/8	3/10
		118	6	40	72	0.220	0.780	6/40	0/2	2/15	4/23
			5.1%	33.9%	61.0%			15%			
Majuro.		23	1	9	13			2/7	0/1	0/2	2/4
35 other islands.		124	9	49	66	0.270	0.730	9/49	0/6	3/18	6/25
			7.3%	39.5%	53.2%			18%			
Totals.		678	36	229	413	0.222	0.778	69/305	0/22[1]	18/99	51/184
Per cent.			5.3%	33.8%	60.9%			22.6%	0.0%	18.2%	27.7%

[1] Subsequent tests showed that the remaining 14 type M of the 36 type M found, were all anti-S negative.

Approximate chromosomal frequencies calculated from the figures in the present survey are: ms (+ mS) = 0.234; ns = 0.651; nS = 0.115.

18/99 (18.2%) were positive, and in type N 51/184 (27.7%) were positive. Subsequent tests showed that the remaining 14 samples of type M of the 36 type M samples found, were all anti-S negative. The approximately chromosomal frequencies calculated from the figures in the present survey were:

$$ms(+mS) = 0.234,\ ns = 0.651,\ nS = 0.115.$$

The frequency distribution is unique. There was no evidence of the chromosome *mS* in this series, which contrasts sharply with the frequency of 0.25 found for this chromosome in whites. From calculations based on the frequencies of the other gene it is likely that the frequency of *mS* is slightly less than 0.01. Graydon, Simmons, Semple, Clapham, Wallace and Harrisson ('51) in a paper dealing with the blood genetics of 5 tribal populations in Borneo have prepared a table showing some of the existing racial data on M-N-S phenotypes and frequencies of chromosomes. To this table will shortly be added new data on various Pacific populations referred to earlier in the text. In the table the approximate chromosomal frequencies for the colored races are distinct from those of white groups, and the frequency of *mS* is low, or zero. Some figures taken from their table show that the S positive percentage in various populations varies as follows: Melanau (Borneo) 3%, Kedayan (Borneo) 16%, Australian aborigines 0%, New Guinea natives 22.7% and 37.0%, Maoris 12.2%, present survey 22.6%, English 57% and 53.5%, Australian whites 59.3% and 48.2%. It is obvious that the chromosomal distribution of S should undoubtedly prove of great value in studying racial origins.

A total of 405 random blood samples from all islands was tested with two anti-M sera capable of detecting the variant M_2 (Jakobowicz, Bryce and Simmons, '49), and two anti-N sera prepared with the blood of two individuals. No variants were detected.

Rh types. Table 4 presents the Rh types and gene frequencies found in 678 Marshallese. All of the samples were

TABLE 4

The RH types and gene frequencies in Marshallese

ISLAND OF ORIGIN AND ATOLL		NUMBER OF BLOOD SAMPLES TESTED	RH TYPES					
			Rh_1Rh_1	Rh_1Rh_0	Rh_2Rh_2	Rh_2Rh_0	Rh_1Rh_2	Rh_0
Kaven,	Maloelap.	50	46	0	0	0	4	0
Tjan,	Maloelap.	35	31	0	0	1	3	0
Airik,	Maloelap.	32	30	1	0	0	1	0
		117	107	1	0	1	8	0
			91.5%	0.9%	0	0.9%	6.8%	
Tabal,	Aur.	66	59	0	0	0	6	1
Aur,	Aur.	59	52	0	0	0	7	0
		125	111	0	0	0	13	1
			88.8%				10.4%	0.8%
Arno,	Arno.	71	64	1	0	0	6	0
Ine.,	Arno.	54	45	1	1	0	7	0
		125	109	2	1	0	13	0
			87.2%	1.6%	0.8%		10.4%	
Alu,	Mili.	30	24	0	1	0	5	0
Lukunor,	Mili.	16	14	0	0	0	2	0
		46	38	0	1	0	7	0
			83%		2%		15%	
Taka,	Ebon.	52	46	1	0	1	4	0
Ebon,	Ebon.	66	59	0	0	0	7	0
		118	105	1	0	1	11	0
			89.0%	0.8%		0.8%	9.3%	
Majuro.		23	22	1	0	0	0	0
35 other islands.		124	122	0	0	0	2	0
			98.4%				1.6%	
Totals		678	614	5	2	2	54	1
Per cent.			90.6%	0.7%	0.3%	0.3%	8.0%	0.1%

Gene frequencies: $R^1 = 0.9516$, $R^2 = 0.0419$, $R^0 = 0.0065$.
All of the 678 samples tested were anti-Rh_0 (anti-D) positive.

anti-Rh_0 (anti-D) positive. The distribution of types was again fairly uniform from atoll to atoll. Of the 678 tested, 614 (90.6%) were of phenotype Rh_1Rh_1, 5 (0.7%) were Rh_1Rh_0, 2 (0.3%) were Rh_2Rh_2, 2 (0.3%) were Rh_2Rh_0, 54 (8.0%) were Rh_1Rh_2, and 1 (0.1%) was Rh_0. The gene frequencies calculated from these figures were:

$$R^1 = 0.9516,\ R^2 = 0.0419,\ R^o = 0.0065.$$

The frequency of $R^1 = 0.9516$ is the highest for any race reported to date. In the tables prepared by Simmons and Graydon ('51) showing the frequency of R^1 and the Rh gene frequencies for various races, it was seen that Australian aborigines, Indians (Moslems), (and more recently Maoris) fall into a group with an R^1 frequency approximately 0.5. The second group, Chinese (Southern), Siamese and Japanese were approximately 0.75. The third group consisting of Marshallese, Papuans, Admiralty Islanders, Filipinos, Borneo (5 groups), Fijians, Indonesians (Java), Indonesians (20 islands), New Caledonians, Loyalty and Pine Islanders, varied from 0.95 to 0.81. A 4th Rh gene R^z detected in a number of the Pacific races was not detected in Marshallese. In this group the 54 samples of phenotype R_1R_2 were tested with anti-hr′ (anti-c) serum and all gave positive reactions.

The Rh gene frequencies for Marshallese are closest to those calculated for Filipinos, Indonesians, Papuans, Admiralty Islanders and Fijians.

A total of 306 random blood samples representing all islands and of all blood groups was tested with a potent anti-C^w serum found in Melbourne. No example of C^w was detected.

A total of 100 blood samples of groups O and A was tested with 5 selected anti-Rh_0 (anti-D) agglutinating sera (Simmons and Graydon, '50). Three of these sera were anti-D and two were anti-D + D^u. No example of Stratton's D^u variant was detected.

Lewis types and secretor factor. Table 5 shows the correlation between the Lewis (Le^a) blood group and secretion of A, B or H substances in saliva. A total of 390 blood samples

TABLE 5

Correlation of Lewis (Le^a) blood group and secretion of A, B or H substances in saliva in Marshallese

ISLANDS, AS IN GROUPS VISITED AND SAMPLES COLLECTED	NO. OF BLOOD AND SALIVA SAMPLES TESTED	GROUP O				GROUP A_1				GROUP B				GROUP A_1B				TOTALS			
		Le (a+)[1] ss	Le (a+) S	Le (a−) ss	Le (a−)[2] S	Le (a+) ss	Le (a+) S	Le (a−) ss	Le (a−) S	Le (a+) ss	Le (a+) S	Le (a−) ss	Le (a−) S	Le (a+) ss	Le (a+) S	Le (a−) ss	Le (a−) S	Le (a+) ss	Le (a+) S	Le (a−) ss	Le (a−) S
Kaven, Tjan, Airik.	68	9	0	2	32	3	0	0	10	3	0	0	7	0	0	0	2	15	0	2	51
Tabal, Aur, Arno.	33					4	0	0	5	6	2	1	14	0	0	0	1	10	2	1	20
Arno, Ine.	57					9	0	1	13	6	0	2	15	2	0	0	9	17	0	3	37
Alu, Lukunor, Taka.	26					2	0	2	12	2	0	1	5	0	0	0	2	4	0	3	19
Taka, Ebon.	27					3	0	0	9	2	0	0	9	0	0	0	4	5	0	0	22
Totals	211	9	0	2	32	21	0	3	49	19	2	4	50	2	0	0	18	51	2	9	149
Per cent.																		24.2%	0.9%	4.3%	70.6%

101 (25.9%) Le(a +) samples were found in a total of 390 blood samples tested, representing all islands as shown in table 1.

[1] Le(a +) = agglutination of blood with anti-Le^a serum. ss — non-secretor of A, B or H substance in saliva.

[2] Le(a) = no agglutination of blood with anti-Le^a serum. S = secretor of A, B or H substance in saliva.

The eleven exceptions in the above table were distributed as follows: Kaven 1, Tjan 1, Aur 3, Ine 3, Taka 3. Agreement between Lewis and Secretor tests occurred in 95% of those examined.

were tested with anti-Le^a serum and of these 101 (25.9%) were found to be Le(a +). A total of 211 corresponding blood and saliva samples were tested and agreement between Lewis and secretor status occurred in 95%. The results obtained are set out in the table according to groups of islands and according to the blood groups O, A_I, B and A_IB. There were 11 exceptions and of these two were Le(a +) and secretors (S) and 9 were Le(a —) and non-secretors (ss). The exceptions found may be due to any of a number of reasons:

1. Clerical errors in identifying samples;
2. failure to boil saliva samples adequately, or soon enough after collection;
3. possible deterioration of blood samples before testing;
4. may represent occurrence of crossing over between loci so closely linked that crossing-over equilibrium has not yet been established.

Considering the difficulties under which samples were collected in the field and the lapse of time before the samples were tested in Melbourne, the degree of correlation obtained must be regarded as excellent. The correlation between the Lewis blood group and the ability to secrete A, B or H substances in the saliva which was first observed by Grubb ('48) has been shown by us to apply to white Australians and all colored races which we have tested to date. In some instances 100% correlation has been obtained.

The agglutinogen P. A total of 100 random blood samples representing all islands was tested with an anti-P serum. Of these 80 were regarded as P positive.

Duffy and Kell blood groups. In recent months we have received gifts of anti-Duffy "blocking" serum from Dr. P. L. Mollison (England), anti-Kell agglutinating serum from Mr. I. Dunsford (England), and anti-Duffy and anti-Kell "blocking" sera from Dr. A. S. Wiener (U.S.A.). We gratefully acknowledge these generous gifts. The reactivity of the respective sera have been compared by testing numerous individuals on our panel. Mollison's "Duffy" serum was used undiluted while Wiener's "Duffy" was used diluted 1/5 in saline; both gave excellent and comparable positive agglutination reactions when the sensitized cells were tested with a potent Coombs reagent on slides. The tests were read after 5–10 minutes at room temperature (20°C.). Both sera were completely specific. Dunsford's anti-Kell agglutinating serum of group O was used undiluted on slides versus an equal part of 10% cell suspensions in glucose-citrate solution, and the reactions read after one hour at 37°C. Wiener's anti-Kell was used diluted 1/3 in saline and the sensitized cells subjected to the Coombs test. Both sera gave excellent and comparable Kell positive reactions according to the expected percentage. We are of the opinion, however, that each serum contains an extra and possibly identical weak antibody capable of reacting in varying strengths with all cells, if the time of reading the agglutination or Coombs test is delayed sufficiently long. By carefully standardizing technique difficulties in determining true Kell positive reactors were overcome. As stated earlier in the text the Marshallese blood samples were collected between April and June, 1950, and were tested during July, 1950. On completion of all the tests which it was possible to do at that time, the samples were stored at 5°C. in a cold room and remained there for a further 13 months until September, 1951. The age of the samples then was approximately 16 months.

It was decided to test some of the 16-month-old Marshallese blood samples, if possible, for their "Duffy" and "Kell" blood groups. Thirty blood samples were taken from random boxes of the Marshallese samples. All samples selected had an intact layer of sedimented red cells with dark-colored supernatant fluids. This fluid was removed and the cells were washed once in saline and resuspended to 20–30% strength in glucose-citrate solution. They were stood overnight at 5°C. before using. None of the samples showed any evidence of haemolysis during the performance of all subsequent tests. Each sample was A-B-O grouped, subgrouped and Rh typed as unknowns, and the results obtained agreed with the original results. No difficulties whatever were encountered in reading the results, and agglutinations with A, B, O, Rh_o, rh′, rh″ potent sera were practically equal to those obtained with fresh cells, but agglutinations with anti-A_1 and anti-hr′ were about one-half the original strengths.

''Duffy'' and ''Kell'' tests were then performed on the 30 samples with adequate controls, and again no difficulties were encountered. The results showed that 30/30 were ''Duffy'' positive and 30/30 were ''Kell'' negative. It is now proposed to test along the same lines small groups of various samples received by us during the past 15 months, and to ascertain the ''Duffy'' and ''Kell'' reactions in those native groups from which, in some instances, it would be impossible to obtain further samples.

It may sound fantastic that 16-month-old blood samples can be subjected to satisfactory blood grouping test, but we can assure readers that the above results are accepted and presented by us as entirely above reproach.

DISCUSSION

Although there is a paucity of anthropological data it is generally agreed that the Micronesians are a very mixed population. In his comments Birdsell, after emphasizing this lack of adequate data on which to build rational theories, has discussed his own and the views of others concerning the components involved in this racial complex.

The various views may be summarized briefly:

1. Howells — Allied to the Polynesian with a strong Indonesian influence.
2. Hasebe — Negroid, a small generalized type, Polynesian and Indonesian Mongoloids.
3. Birdsell — Negritoid, Caucasoid (Ainoid), and Mongoloid basic components.
4. Avias — Melanesian, Indonesian, Polynesian and recent Asiatic (Malay, Chinese, Japanese).

At the present stage of our work it would seem unwise to attempt critical analysis of the foregoing theories on the basis of the blood genetic results presented in this paper. Such analysis should await the results of further surveys of Micronesian and relevant groups which should soon be available for publication

Tentatively and with considerable reserve we would make the following observations based on published blood grouping data some of which are given in table 6.

1. No simple Polynesian-Indonesian mixture could produce frequencies for the A-B-O, M-N and Rh systems similar to those for the Marshallese.
2. If, in commenting on Hasebe's theory, it is assumed that the Negroid and small generalized type to which he refers were the major components in the Melanesian complex, we would expect to find frequencies in the Marshalls close to those observed in Melanesia, but with bias toward Polynesian and Indonesian frequencies. Our data indicate that the Marshallese and Melanesian (Papuan) frequencies are strikingly similar and that the former do not show the expected additional evidence of Polynesian and Indonesian admixture.
3. The same comparisons with Melanesian blood group frequencies suggest that the rather strong Mongoloid influence in Micronesia advanced by Birdsell has left little mark on the blood group frequencies in the Marshalls.

TABLE 6

Gene frequencies

POPULATION	GENE FREQUENCIES									
	p	q	r	m	n	R^1	R^2	R^0	R^z	r'
Marshallese	0.135	0.134	0.723	0.222	0.778	0.9516	0.0419	0.0065	0	0
Polynesian (Maori)	0.338	0	0.662	0.525	0.475	0.465	0.486	0.027	0	0.022
Indonesian (20 islands)	0.130	0.133	0.737	0.435	0.565	0.81	0.13	0.04	0.02	0
Melanesian (Papuan)	0.215	0.127	0.674	0.227	0.773	0.943	0.020	0.021	0.016	0
Chinese	0.165	0.139	0.681	0.630	0.370	0.760	0.195	0.040	0.005	0

Three of these basic components have been mentioned by him as contributing to the Melanesian complex but with a different emphasis on their respective importance. The differences are not reflected in our results to date. Birdsell has suggested (Simmons, Graydon, Avias and Williams, '49) that the Negrito component is dominant in the Melanesian Islands, e.g., New Guinea, Papua. If the predominant racial type is Negritoid then this type, according to our blood group frequencies, predominates both in Marshallese and coastal Papuans, and admixture with other racial components has not greatly altered the blood group frequencies of either group.

4. The same remarks are applicable to the suggestion put forward by Avias. He refers, however, to populations and not to basic racial components.

SUMMARY

1. A total of 678 Marshallese samples were tested for the A-B-O groups, and 354 (52.2%) were of group O, 145 (21.4%) were of group A, 143 (21.1%) were of group B and 36 (5.3%) were of group AB. The gene frequencies were: $p = 0.135$, $q = 0.134$, $r = 0.723$. All of the 181 individuals of groups A and AB were of subgroups A_1 and A_1B.

2. In tests on 678 samples, 36 (5.3%) were of type M, 229 (33.8%) were of type MN and 413 (60.9%) were of type N. The gene frequencies were:

$$m = 0.222 \text{ and } n = 0.778.$$

Of 305 samples tested for the S subdivision of M-N, 69 (22.6%) were anti-S positive. The chromosomal frequencies were:

$$ms(+mS) = 0.234,\ ns = 0.651,\ nS = 0.115.$$

Of 36 type M samples found all were anti-S negative.

3. The Rh types found in 678 samples were:
Rh_1Rh_1, 614 (90.6%); Rh_1Rh_o, 5 (0.7%); Rh_2Rh_2, 2 (0.3%); Rh_2Rh_o, 2 (0.3%); Rh_1Rh_2, 54 (8.0%); Rh_o, 1 (0.1%).
The gene frequencies were:

$$R^1 = 0.9516, R^2 = 0.0419, R^0 = 0.0065.$$

All of the 678 samples were anti-Rh_0 (anti-D) positive.

4. Of 390 blood samples tested with anti-Le^a serum, 101 (25.9%) were Le(a +). There were 211 corresponding blood and saliva samples tested and the correlation obtained between the Lewis blood group and salivary secretion of A, B or H substance was 95%.

5. Tests were made for M or N variants in 405 random blood samples. No variants were detected.

6. No D^u variants (Stratton) were detected in 100 blood samples when tested with 5 selected anti-D and anti-D + D^u sera.

7. A total of 306 random blood samples were tested with a potent anti-C^w serum. No C^w variants were detected.

8. Of 100 random blood samples tested with anti-P, 80% were regarded as P positive.

9. Marshallese blood samples 16 months old were tested under properly controlled conditions with recently acquired sera for the "Duffy" and "Kell" blood groups. Thirty random samples were all "Duffy" positive, and all were "Kell" negative.

10. References to recent Australian blood genetic surveys conducted from these Laboratories in the Pacific area have been given.

11. The gene frequencies found for the Marshallese have been briefly discussed in relation to the views expressed by several writers concerning the racial complex of this population.

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HUMAN CONSTITUTION: AN APPRAISAL

EDWARD E. HUNT, JR.
Forsyth Dental Infirmary for Children, Boston

The aim of this paper is to explore the concept of human constitution and to examine a few recent ramifications of constitutional research in the English-speaking world, especially in relation to anthropology as a whole. To me, constitution is a point of view rather than a separate discipline, and is part of a modern shift in anthropological awareness from the group toward the individual.

As an historical and social science, the broader view of cultural anthropology is expressed in concepts such as ''diffusion'' and 'society.'' In physical anthropology, the corresponding concern is with human populations, their physical evolution, gene distributions, anatomical peculiarities, and patterns of physical growth, maturation, and aging.

On the other hand, a more restricted view of anthropology is now emerging. On the cultural side, this trend is particularly evident in studies of biography, personality, and the limits of human behavior and values within one culture. Here the emphasis is on the individual as a type rather than on the group as the focal concept. The parallel trend in physical anthropology adds the structure of the individual to the picture, and concerns itself with problems of genetics, ecology, medicine and psychology. This constitutional viewpoint never neglects the shape of the body, and as such, is virtually physical anthropology in miniature.

In cultural anthropology, the concept of individual ''personality'' merges into ''national character'' or ''modal personality.'' A corresponding merger can be seen in phylogenetic and constitutional studies of the human body.

On the phylogenetic side, although the antiquity and lineages of man's fossil ancestors are uncertain, all parts of the body seem not to have evolved at the same rate. In a real sense, the head, brain and face probably became human later than the trunk and limbs. In addition to these unbalanced rates of evolutionary change, great structural plasticity has prevailed within breeding populations of both mankind and our surviving simian kin. We cannot explain this variability in many parts of the body in the higher primates without an understanding of individual variations. In this kind of inquiry, the focus of the evolutionist must shift from the population to the individual, and from history to biography.

A concept equally applicable to evolution, to human growth and aging, and to constitution is what I call the ''ontogenetic scale'' of variations between two extremes of structure, functioning, and behavior. At one extreme is an evolutionary change which increases the amount of alteration of a body part or function during growth. This change leads to a pronounced adult specialization of pattern. At the opposite extreme is a change which decreases the amount of this alteration during growth. This decrease leads to a markedly infantile configuration, but not necessarily a small body size, in the adult.

This kind of scheme has long been applied to trends in the physical evolution of animals by many students, notably de Beer ('38) in recent decades. In man, the physical modifications toward the infantile end of the scale have been examined by the Dutch anthropologist Bolk ('29). Some of these features include internal (Mongoloid) eyefolds, relatively prominent eyeballs, light skin and hair color, blue eyes, and a flattened nasal bridge. Infantile features of body build in the adult include a deep chest, prominent belly, and short limbs. All of the foregoing structural peculiarities are found in late fetal life and in the newborn human infant.

Features at the opposite or adultiform pole of the ontogenetic scale in man include prognathism, deepset eyes, a prominent nose, dark pigmentation, a broad chest, flat belly, massive bones and muscles, and long extremities.

In the course of time, some evolutionary changes in a lineage of organisms are toward the infantile end of the scale, and others are in the opposite direction. For example, relative to the other primates, most of the human head has evolved in an infantile direction. It retains the relatively large braincase and lack of projection of the jaws which are found in all infant mammals. The gorilla is a noteworthy contrast. Its head shows adultiform prognathism, and its musculature and skeleton are mainly adultiform as well. It retains the large viscera, bulging belly, deep chest and short lower limbs of infancy, however, and in these respects is more infantile than man.

Within the human species today, the Negritos, especially those of Southeast Asia, are relatively infantile in most of their structure. Other human races are inconsistently infantile or adultiform in various parts of the body. Most of the Whites, for instance, are adultiform in their prominent noses and massive skeletons. They are infantile, on the other hand, in their light pigmentation and lack of prognathism of their jaws.

As was mentioned earlier, constitution is in part the study of the evolutionary microcosm, and the ontogenetic scale is as applicable to individual variations in physique and personality as it is to the more general field of human phylogeny. This kind of thinking has an honorable past in constitutional theory. Some of the parallels of human growth stages and adult physiques have been noted by a number of constitutionalists, especially Bean ('26) and Naccarati ('21).

The most infantile build in the human adult retains the rounded trunk and limbs and large viscera of the newborn throughout life. In the terminology of Sheldon, such physiques are called *endomorphs*. A less infantile build is the *ectomorph,* who exhibits the lean frame and attenuated limbs of the prepubertal years all his life. The most adultiform build is distinguished by heavy bones and muscles throughout life. These features develop less markedly in other physiques during the growth spurt of early adolescence, and boys show them more markedly than girls. This adultiform variety of human physique is the *mesomorph.* Most human bodies are intermediate and are scaled as such with ratings of from 1 to 7 with the central tendency at 4, in each of the three morphological components: endomorphy, mesomorphy and ectomorphy.

The ontogenetic scale as applied to human psychological variations is equally suggestive: particularly in the formulations of Freud and of Sheldon himself. The infantile personality can be identified with oral drives or viscerotonia in the adult; the prepubertal stage of mental development with anal and cerebrotonic patterns in later life; and the adultiform personality with somatotonia (Hunt, '49).

The concept of the ontogenetic scale does not assume that all parts of the body or total personalities must become consistently infantile or adultiform, whether in evolution, physical growth, or the development of personality. Disharmony is one of the most characteristic phenomena found in human individuals, and its measurement and interpretation are a formidable intellectual challenge.

The ontogenetic scale is the most holistic and inclusive notion in constitutional research today. When we turn to more operational and limited areas of discourse, constitutional thought is not so completely formulated. Three formidable obstacles still impede progress in this field: inept classificatory concepts, careless research, and unfounded value judgments on the alleged vices of some human physiques and temperaments.

The classification of body build is still one of the most controversial problems in constitutional research, even though much better methods of appraisal are used today than were known 15 years ago. The somatotype, as first described by Sheldon, Stevens and Tucker ('40), is still the focus of this controversy. These authors state that if nude photographs of the body build of one individual are taken on any single occasion, skilled observers can estimate ratings of his endomorphy, mesomorphy and ectomorphy from the pictures alone. These ratings are supposedly not only fixed for life, but are determined within very narrow limits by the individual's endowment of genes.

In my opinion, the skill necessary to derive such fundamental ratings is still impossible to acquire. For example, Lasker ('47) showed that the ratings of somatotype photographs of the same individual taken before and after partial starvation are usually quite unlike.

The actual operations of somatotyping from photographs are fundamentally those of form recognition, and ratings are derived from both conscious and unconscious cues in the photographic images. Quite similar cognitive processes occur in the identification of strange aircraft or the judging of prize animals at a dog show. These activities can usually be carried on with considerable consistency among skilled observers of any of these phenomena.

Although evidence on the heredity and nurture of human somatotypes is still indecisive, work on body build in horses, sheep and pigs, notably that of Hammond ('50), indicates that animals of different breeds can be made to attain markedly similar adult shapes by experimenting with their caloric intake at various stages of growth. These physiques are attained, however, at exceedingly different rates of maturation. This evidence suggests that two human beings may attain the same adult somatotype through the operation of quite dissimilar genes and environmental influences. The differences in physique mentioned by Kallmann ('48) in monozygotic twins also suggest that the somatotype is not wholly determined by heredity.

Recognizing the semantic chaos resulting from the original formulation of fixed somatotype ratings, Sheldon, Hartl and McDermott ('49) now choose to differentiate between the "morphophenotype" (component ratings taken from body build photographs made on one occasion) and the "morphogenotype,'' which is an extrapolated unalterable construct of component ratings which are fixed for life by the genes.

In my opinion, this distinction is still far from a satisfactory formulation of somatotyping. Since the attempt to rate the morphogenotype of an individual is highly subjective, has few operational referents, and is impossible to validate, more precise but arbitrary methods of appraising body build are becoming desirable. Sheldon's three components, or their approximate equivalents, may still be used descriptively, but other modes of analyzing physique are by no means invalidated.

The appearance of a person as seen in body build photographs taken at any one time should be regarded as a momentary state, comparable to his responses to one Rorschach test or the symptoms of his current toothache. Even at the risk of forcing our ratings of body build into a Procrustean bed, a relatively fixed rating system, applicable at least to all adult ages, has the advantage of greater objectivity and simplicity than the typical operations of Sheldon. Such a method is now being developed by Professor E. A. Hooton and his associates. It retains 3 component scales of 7 points, somewhat similar to those of Sheldon's somatotype, but differs in both procedures and premises from the rather exuberant presuppositions of Sheldon.

Hooton's third component, which he calls ''attenuation,'' is measured by the use of equal intervals on a scale derived from the ratio of stature divided by the cube root of weight. The degrees of attenuation delimited by these intervals range from 1 to 7, much as do the ratings of Sheldon's ectomorphy.

One of Hooton's colleagues, Frederick L. Stagg, has a distinctive interpretation of the stature/$\sqrt[3]{\text{weight}}$ index. To Stagg, it measures bodily ''density'' rather than attenuation. This index would literally be proportional to the reciprocal of density if physiques of all sizes and ages were shaped alike. It is biased, however, by the variability prevailing in human contours and proportions, and is far from an accurate measurement of density during growth.

Dupertuis ('50) found that Sheldonian endomorphy shows a high negative correlation (— .853) with specific gravity: whereas ectomorphy shows only a moderate positive correlation (+ .369) with this measurement. Since Hooton's intervals of the height/$\sqrt[3]{\text{weight}}$ index are not far from the rating of ectomorphy from photographs, and since density is proportional to specific gravity, evidently this index is not as close an approximation to density as the reciprocal of an endomorphic or fat component might be.

Furthermore, the confusion of the height/$\sqrt[3]{\text{weight}}$ index with density is not a rigorous use of a measuring scale derived from physics. Yet human beings do differ in true density, as shown by their varying ability to float motionless in a swimming pool. This variability in density makes the index of height/$\sqrt[3]{\text{weight}}$ a somewhat biased measurements of attenuation. Unlike "density," however, "attenuation" is a term without a precise definition derived from physics. It is therefore vague enough to apply rather aptly as a label for Hooton's third component.

In the system of Hooton and Stagg, the first component of physique is simply called "fat," and the second "muscularity." These two components are usually derived from the study of nude photographs, much as endomorphy and mesomorphy are rated by Sheldon, while the third component is derived from the index of attenuation mentioned earlier. Obviously, the ratings based on Hooton's method change with age, body weight, and probably diseases, exercise and diet.

At present, Hooton and his colleagues are working out methods of deriving the fat component in adult males from simple unweighted metrical indices of physique. Most of these indices are based on ratios of circumferences to lengths of body parts. These metrical ratings are already close to those which he and his associates find from the study of photographs. Such ratings will probably become very valuable, especially in future studies where nude photographs may be impossible to take.

The muscular component so far has not been so successfully rated from measurements. A solution to this problem may eventually be found through the use of discriminant functions.

Hooton finds that his present method is easier for students to learn than Sheldon's technique. Hooton's approach represents an unwillingness to dogmatize on the respective roles of heredity and environment, and is somewhat more operational and noncommittal than Sheldon's contentions. It should therefore be regarded as only a beginning. The only way to build a secure and well-founded science of human body build is through the arduous and protracted task of longitudinal studies of individuals throughout life, with a photographic history covering the whole cycle of growth, maturity and senescence. The morphological ratings and other characteristics of an individual physique at all ages could then be plotted as trajectories, much as are the channels of growth on the Wetzel grid. We can probably arrive at much more precise knowledge of the heredity, nutrition, and life cycles of different physiques by the longitudinal studies of photographs and roentgenograms than by any conceivable short cut. To a large extent, the understanding of somatic biographies is the beginning of constitutional wisdom.

The conceptual confusion underlying somatotyping, and our limited knowledge of physiques other than at the young adult years, have led some students of constitution to use fewer subdivisions of body types than those derived from Sheldon's 7-point scales. One method is to match individuals or samples by criteria other than body build, and then to compare their balances or dominances in the somatotype components. Gertler, Garn and Sprague ('49) used this device in matching men with and without coronary artery disease. Seltzer has employed it in the Grant Study, a very important study of constitution in "normal" Harvard students (Seltzer, Wells and McTernan, '48), as well as in work on delinquent and nondelinquent boys (Glueck and Glueck, '50). A few crude subclasses of body build were principally used in these projects such as "dominant mesomorphs," "balanced types," and the like. Kallmann ('48) matched monozygotic twins in this way, and found that the first of a pair of such twins to become schizophrenic was the shorter and more ectomorphic of the two.

Component ratings are only part of an adequate armamentarium for studying human body build. Other techniques of morphological appraisal include tissue distributions, factor analysis, and the measurement of sexual dimorphism. For example, the thickness of fat and bone in the calf of the leg can be seen in suitably exposed x-ray photographs. The work of Stuart and Sobel ('46) and of Reynolds and Wines ('48) is especially noteworthy in this field. The depth of subcutaneous fat can be estimated from the measurement of skin folds with calipers. The most exhaustive investigation of these folds to date is that of Edwards ('50). He has measured skin folds in over 50 regions of the body in adults of both sexes. This technique is especially promising for the appraisal of weight reduction and obesity.

Factor analysis is becoming an important tool of physical anthropology because of an inherent weakness of traditional anthropometry. Ordinary physical measurements have contributed little to constitutional thought because they have provided no sophisticated or holistic view of human body build. Instead, the measurement of racial or other samples of human beings usually leads to tables of central tendencies, variability, and perhaps the intercorrelations of measurements and indices. Such tables do not tell us whether a single, potentially isolable developmental process may account for more than one dimension of the human body. Moreover, genetics and experimentation have so far provided only a few fragmentary insights into such unitary processes.

A similar predicament in evaluating multiple test scores in psychology led to the development of factor analysis two decades ago. This technique is an ingenious use of mathematics which simplifies and clarifies tables of correlations. It is a method applicable to correlations taken from any field of science, including anthropometry. Howells ('51) has reviewed the numerous attempts to apply factor analysis to the study of human physique. Among the workers who have made such attempts have been Tanner ('47) and Burt in England, and Thurstone, Marsh and Howells in the United States.

Factor analysis is derived from descriptive statistics rather than from the theories of probability, sampling, and tests of significance. In anthropometry, the analysis begins with a table whose columns and rows are both labeled with the designations of all of the measurements which are to be correlated. Each cell in the table contains a product-moment correlation coefficient between the two measurements appropriate to its column and row.

This table, or correlation matrix, is put through a series of mathematical transformations. Each transformation produces two results: a factor or vector which accounts for part of the observed correlations in the matrix, and a new matrix of reduced correlations or residuals. Another factor is then extracted from the residual matrix, and this process may be continued through further transformations. As the factors are extracted, one after another, the correlations occurring in the matrices approach zero until only minute residuals finally remain.

The point at which factoring computations should be completed is a matter of disagreement among the factor analysts themselves. Another disputed issue is whether a set of factors should be statistically uncorrelated. In a way comparable to the extraction of multiple roots from simultaneous exponential equations, the nature and number of the factors derivable from a matrix are somewhat affected by the subjective judgment of the investigator. In my opinion, it is still too early to say whether this subjectivity of factor analysis will obfuscate rather than clarify our understanding of human growth and aging.

Another use of statistical ingenuity in evaluating human physiques is the unpublished work of Tanner on sexual dimorphism and androgyny. His method is called discriminant functions. A discriminant function is an equation of maximal efficiency which weighs the importance of various measurements or scores in assigning an individual to a particular group which he most resembles (Tiedeman, '51). It is a technique which would be admirably suited to improving the efficiency of sexing pelves or other bones from raw measurements. Tanner is using it initially to discriminate metrically between men and women, but ultimately to derive an objective scale of androgyny. In my opinion, this method is more rigorous than the scales of gynandromorphy devised by Sheldon, Stevens and Tucker ('40), and further elaborated by Sheldon, Hartl and McDermott ('49).

A third important method of evaluating physiques is the work of Seltzer on body disproportions, as measured by extreme values of numerous anthropometric indices. His researches have included both work on the Grant Study series (Heath, '45) and the delinquency project mentioned earlier (Glueck and Glueck, '50).

The evaluation and measurement of body build is greatly facilitated by new and exact methods of posing human beings in standardized photographs. The best of these photographs can be measured about as accurately as living individuals (Tanner and Weiner, '49). The ultimate purpose of all such new techniques of measurement and appraisal should be their use in describing growth and aging by as many developmental trajectories as possible. Along with these descriptions will undoubtedly come new and fruitful interpretations of the physiological and biochemical mechanisms at work in the human life cycle, and their interrelationships with physique, diseases and behavior.

In the planning of research from the constitutional viewpoint, problems of sampling and the universality of generalizations are as thorny as in any other quest for knowledge. Constitutional research in the past has been so full of shortcomings in these respects that many critics have called it worthless. The most careful and rigorous methods of investigation are necessary to answer these criticisms. Fortunately, some attempts in this direction are already showing up in the recent literature.

Imperfect sampling controls have weakened many constitutional investigations in the past, but even some of the older workers dealt with sampling skillfully. Kretschmer ('36), for instance, carried out his studies of physique and psychoses almost entirely on Swabian farmers. To some extent, the diversity of national extractions in the series studied by Sheldon and Stevens ('42) in deriving their conceptions of temperament can be regarded as an incomplete experimental control of both genetic and cultural variability.

The best sampling designs in constitutional research, in my opinion, involve the choice of samples from one human breeding isolate with a relatively homogeneous culture, or the comparison of individuals by criteria which match them both by national extraction and by cultural background. The first method has precisely the same advantages as the ethnographic study of a small human group: it is possible to find the limits of both physical and behavioral variations within a manageable cultural compass. An example of this ethnographic approach is my own work on the social classes, crimes and somatotypes of the Micronesians of Yap, reported in the 1951 proceedings of the American Association of Physical Anthropologists (Hunt, '51).

The comparison of individuals by the matched-pair technique is one of the more efficient methods of exploring constitutional variations in civilized societies. Glueck and Glueck ('50), for example, compared 500 delinquent and 500 non-delinquent boys with respect to many physical, biographical and psychological variables. In this case, each delinquent boy was matched with a non-delinquent for age, national extraction, intelligence quotient, and residence in comparable underprivileged parts of Greater Boston.

Another case of sampling where environment is partially controlled is the current research of Stagg in Hooton's laboratories. Stagg is studying over 2,000 body build photographs taken, in college, of Harvard men who graduated around the turn of this century. This group was probably more homogeneous in heredity and culture than Harvard students of today. Stagg is comparing the young adult physiques of his subjects with their entire life histories. The great value of this project not only lies in its discovery of relationships between behavior and physique, but also in its clarification of patterns of life which are independent of body build. The elimination of irrelevancies is nearly always an indication of scientific progress. Like of the work of the Grant Study, Stagg's research may define boundaries which can help to delimit the scope of future constitutional studies.

Sampling by occupations, too, is somewhat advantageous, as in the work of Hooton ('48) on the somatotypes and occupational specialties of more than 40,000 soldiers in the United States Army. Here genetic controls are lacking, but the data indicate that physique is related to both civilian and military occupations in this country.

A serious shortcoming of much constitutional research in the past has been the evaluation of both the structure and functioning of an individual by the same investigator. Insofar as these two evaluations are subjective and contemporaneous, the observer's preconceptions on build and behavior may seriously bias his findings. The very high correlations of somatotype and temperament recorded by Sheldon and Stevens ('42) are still somewhat suspect because of Sheldon's own failure to separate his morphological and temperamental analyses. Temperament was described and rated on the basis of prolonged interviews and observations of the behavior of over 200 subjects. If an investigator is interested in the relationship of build and personality, it is obviously impossible to avoid some kind of judgment of a subject's physique during an appraisal of his words and actions during an interview. The only way out of this dilemma is to rate temperament or personality through psychological tests or other personal documents which avoid morphological evaluations entirely. Indeed, unimpeachable research in constitution is impossible without the extensive use of independent sources of data on physique and personality.

In the past few years, students of constitution have become concerned with sources of bias or halo effects, and several methods of separating various parts of the data have been used. Sheldon, Hartl and McDermott ('49) found that a skilled observer can diagnose psychotic trends in an unknown mental patient while the subject is being photographed in the nude for later somatotyping. In this brief and standardized situation, Sheldon's diagnoses usually resembled closely those made after long and independent study by hospital psychiatrists.

Another use of independent judgements in constitutional research is the recent work of the Gluecks on juvenile delinquents which was mentioned earlier. No less than 35 staff members collaborated on this project, but did most of their work on various psychological, biographic and morphological problems separately and independently. Among the most important findings were the relative prevalence of both mesomorphy and somatotonia in the delinquents, and the fact that somatotonia could be discerned not only in the case histories, but also in Rorschach test protocols when Dr. and Mrs. Ernest G. Schachtel interpreted individual responses to this test without knowing the somatotypes of the boys.

A third case of such independence of effort was my own study (Hunt, '51) of physique, social class and crimes in Yap men, discussed earlier. The histories of crime were collected from the police records on Yap by a Marine Corps sergeant, their body builds were evaluated by an observer who knew nothing of the class affiliations of the men whom she somatotyped, or even whether or not they had criminal records. The social classes of the men were derived from an independent census of Yap, and the statistical tables were my own work. In this study, associations were found among short stature, mesomorphy, and membership in a subordinate tenant group somewhat like one of the lower Hindu castes. Violent crimes and mesomorphy were likewise associated, but no relationship could be found between the class position of the men and their criminal records. Furthermore, criminals and non-criminals as a whole could not be distinguished by the incidences of their somatotypes.

The constitutional viewpoint is based on the elucidation of human differences within populations or cultures. In the same way, one of the foundations of social theory is the raw material of field studies in ethnography and sociology. But inclusive generalizations in the sciences of man require the testing of the same hypotheses in many populations and cultures, and some of the more enterprising students of constitution are already examining the linkages of physique and personality in societies other than our own. Charles Morris is now at work on the relationships of physiques, psychoses, and orientations toward ethical goals and works of art in the peoples of China, India, Japan and the United States. This study is already disclosing similar constitutional patterns in all 4 of these nations. It can no longer be said, for example, that the correspondences of physique and temperament found by Sheldon and Stevens ('42) are confined to students in a few American universities. The ubiquity of these correlations, however, remains to be demonstrated in future research on the widest possible diversity of human societies.

In our present state of ignorance, a very real danger exists in evaluating human physiques or personalities, on insufficient evidence, as categorically superior or inferior. Ill-founded recitals of inborn racial virtues and vices still show up in anthropological writings, and egregious constitutional pronouncements are also still appearing, even today. Sheldon, Hartl and McDermott ('49) have made many such value judgments, particularly in their extension of the term "delinquency" to a number of physical and psychological traits and patterns which these authors view with distaste.

Unfortunately, the opinions of constitutionalists on inferior human organisms are not ethically neutral. Masquerades of ethnic hatreds under the guise of science have undoubtedly contributed to the persecution and slaughter of Jews and Slavs in Germany and Eastern Europe. Careless constitutional writings might conceivably be used in the same way as an excuse for mistreating or discriminating against some physical types. To me, both racial and constitutional myths are equally dangerous and ethically indefensible.

Within many animal breeding populations, the individuals closely resemble one another. The structural plasticity of single breeding isolates in man and the anthropoid apes, however, is somewhat unusual. Even the islanders of Yap sometimes approximate the extreme somatotypes found in Americans. To my mind, perhaps the widely divergent assortments of genes in the different members of a human group have been necessary preconditions for the development of its culture. Indeed, the great adaptability of man may depend not so much on the overall superiority of particular somatotypes as on the somewhat specialized and distinctive virtues of each of them. Some fanciful examples may serve to illustrate this point. If the association of imaginative and esthetic sensibilities with ectomorphy has long been typical of our species, perhaps the transmission of wisdom from the old to the young has been fostered by the fortunate preservation of men and women with the "lean and hungry look." Similarly, the oral drives of endomorphs may have led our ancestors to eat strange foods in times of famine, and consequently to survive crises that otherwise might have exterminated them. The combative and resourceful habits of mesomorphs, too, have probably served mankind well through many of the perils of life in the remote past. If most of the overall group trends in human biological evolution have been adaptive, perhaps our constitutional polymorphism has been equally important in preserving our species.

In arriving at an understanding of the evolutionary processes going on in man today, the constitutional viewpoint may profitably ramify into both demography and population genetics. Data on the sexual selection, fertility, viability and mortality of various human physiques are all most desirable, and would be a very rewarding field for the evolutionist to explore.

When constitutional studies have given us a better picture of different human hereditary patterns, life cycles, reproductive capacities, personalities and responses to various environments, much of the recent evolution of man may become clearer. In addition, an appreciation of the interrelationships of morphology and disease even now is helpful in both the diagnosis and management of some pathological conditions. Finally, the tragic miscasting of human beings in life roles beyond their capacities may to some extent be alleviated. All of these considerations, in my opinion, make constitutional research both valuable and worth while.

SUMMARY

Research in the human physical constitution, like the study of individual personalities within any one culture, is centered on the individual. Constitutional research is also a microcosm of human biological evolution. Analogies occur in stages of physical growth, adult physiques, and the growth and variations of human temperament.

The relative importance of heredity and environment in affecting the life cycles of different physiques is still debatable. The assumption that a body build photograph represents a momentary state in the somatic biography of an individual is still probably safer than any attempt to find ratings of body build which are constant throughout life. The arduous study of photographs at successive stages of the human life cycle, and the measurement of trajectories of individual growth and aging, may some day provide us with an unimpeachable basis of constitutional knowledge.

Better sampling, especially in culturally homogeneous societies, independent judgments of constitutional variables, and cross-cultural studies of constitution are all necessary steps in arriving at broad generalizations on the physical bases of different human functions and modes of behavior. Ultimately, constitutional knowledge may contribute much to our understanding of evolution, demography, diseases, and individual human potentialities.

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AGE CHANGES IN BODY BUILD

RUSSELL W. NEWMAN
Quartermaster Climatic Research Laboratory, Lawrence, Mass.

SIX FIGURES

INTRODUCTION

Before somatotyping can be effectively utilized for personnel selection or like purposes, the question of the permanence of the somatotype must be investigated. The original source of data on this system of body build analysis (Sheldon, Stevens, and Tucker, '40) considered the problem briefly and concluded that the somatotype was permanent in the normal male. This concept has been adopted by followers of their system. Dupertuis ('51) says: "one of the premises of the constitutional anthropologist is that the components of an individual's somatotype remain constant, at least after skeletal growth is completed." Everyone will agree that the final test of immutability of the somatotype can only be made by following a large number of subjects for many years. Results of this type of investigation have not yet been published, and it may be many years before sufficient material can be collected. In the interim there is another approach, admittedly not as precise, which can give valuable data. This involves a cross-sectional study of a large number of individuals of varying ages. This paper attempts to explore the question of body build permanence through a study of such age groups.

MATERIALS AND METHODS

The data were obtained during an anthropometric survey conducted in 1946 by the Quartermaster Corps at Army separation centers. Approximately half of the separating service men who were measured were also photographed in the standard somatotype poses. These photographs were later typed for body build and studied at Harvard University under the direction of Professor Hooton. Two analyses of the body build data have been completed (Hooton, '48, '49). The body build card for each individual forms part of the anthropometric files at the Quartermaster Climatic Research Laboratory, Lawrence, Mass., and, therefore, were available for machine sorting and tabulation.

Although comparisons will be made with other published series, it is to be noted that the body build analyses carried out on the Army data by Hooton and his associates depart somewhat from the "classical" method outlined by Sheldon. Hooton has not used the Sheldonian terminology of endomorphy, mesomorphy, and ectomorphy but referred merely to the 1st, 2nd, and 3rd components; that terminology will be followed here. The 1st component as assessed by Hooton differs from the Sheldon system only in scoring. Hooton's ('48) ratings of fatty development in the lower grades are more "liberal" than Sheldon's. Conversely, in the Hooton system muscular development is marked more strictly and more conservatively, resulting in a difference of about one grade in the 2nd component. The 3rd component shows the greatest departure from the Sheldon system. Hooton substitutes the familiar ponderal index of height/$\sqrt[3]{\text{weight}}$ for ectomorphy. The range of indices found in approximately 40,000 men was subdivided into 7 equal intervals to give 3rd component ratings. One final difference is that the Hooton system does not recognize the limitations of the sum of the components, 9 to 12, set forth by Sheldon. This will be apparent when the mean component scores of age groups are examined.

The differences between series somatotyped by the Sheldon and Hooton techniques complicate comparative problems. However, if the Hooton system is consistent and differs only in degree, an age group analysis of it should apply equally well to the Sheldon technique.

It would be a difficult and uncertain task accurately to assess the change or lack of change in body build by examining the somatotype frequencies found in each age group. Some 126 somatotypes have been identified in the Army series with only one type, 4-3-4, exceeding a frequency occurrence of 10% in the sample. Over 80% of the types have frequencies of less than 1%. Random fluctuations from age group to age group would greatly complicate this type of analysis. Hooton has a recent paper which seeks to analyze age changes in 18 body type groupings by which he pools various somatotypes into a workable number (Hooton, '51). Even with this lumping technique, some of the group frequencies are very small for careful analysis.

A further complication lies in the uneven distribution of ages in the military sample. However, if the components are really stable in time, the relationship between the components should remain virtually unchanged except for chance variations when different age groups are compared. This enables the investigator to keep the size of the series large enough to give valid results by comparing only items to which every individual contributes.

Thus the method used here involves, among other things, a study of the correlation coefficients between components on a year by year basis. The Army series available for this purpose did not contain equal samples of every age group. The total White male series was not utilized because the number of individuals in each yearly age group rapidly decreased after the middle 30's. Sufficient men were available from 18 through 35 years to give reasonably accurate results. The number of individuals used in each age group are given in table 1. The biographical data gathered on these men recorded age to the last birthday, and the figures that follow are calculated and plotted against the midpoint of each year.

RESULTS AND DISCUSSION

Component ratings. Although the difficulties of following complete somatotypes have been mentioned, it is possible to examine individual component ratings in a simplified form.

The extraction of one component rating from the total body build score is provisionally justified as necessary for graphic representation.

Figure 1 presents the calculated lines of best fit for the percentage occurrences of each rating in the three components by age groups. Each line is labeled with the appropriate component score. Not all possible ratings are represented, since 1's and 7's (with the exception of 1's in the 3rd component) did not occur with sufficient frequency to permit inclusion.

TABLE 1

Age group frequency of 38,440 White males utilized in age change analyses

AGE	NUMBER	AGE	NUMBER	AGE	NUMBER
18	446	24	1438	30	2075
19	5122	25	1661	31	1253
20	11,618	26	1626	32	622
21	2417	27	1366	33	465
22	1977	28	1823	34	384
23	1679	29	2165	35	303

It will be noted that of the five lines appearing in the 1st component graph, the 4's are not closely associated with any other rating in frequency and appear a pivotal group. The complementary scores on either side of this midpoint rating, 3's and 5's, and 2's and 6's, intersect each other in what seem paired complementary relationships. It would appear that if a change occurs in this component, it is a gradual overall shift.

The 2nd component diagram shows a different type of pairing. The 3's and 4's, and 2's and 5's intersect each other in complementary fashion, and the 6's stand alone. One change also appears in the slope of the lines; the 4's increase with time in this component in contrast to the 1st component, but the other ratings retain the same general trends.

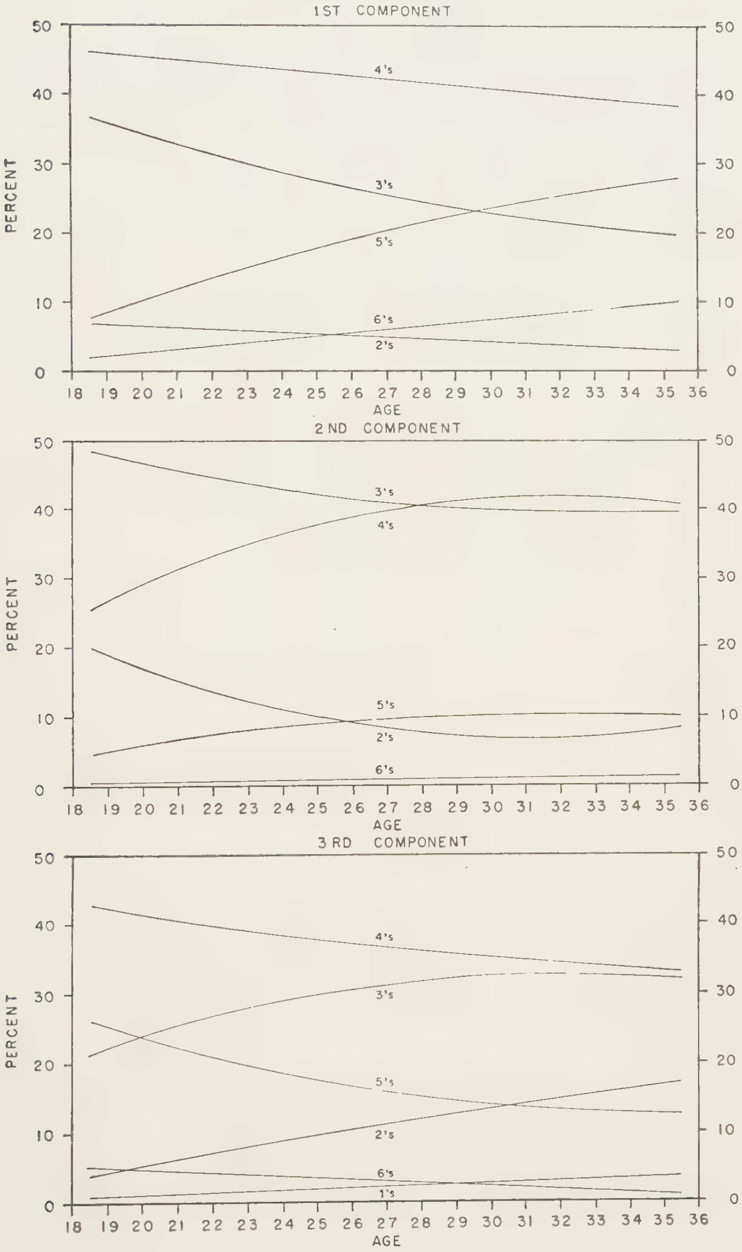

Fig. 1 Percentage occurrence of component ratings for ages 18 through 35.

The positions of the 3rd component lines more closely resemble those of the 2nd than the 1st component in pairings. Again the pairings are 3's and 4's, and 2's and 5's. The slopes, however, are largely reversed in direction. The reason for this will become apparent when the mean values are analyzed in figure 2.

TABLE 2

Mean component values by age group

AGE	1ST COMPONENT		2ND COMPONENT		3RD COMPONENT	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
18	4.07	0.85	3.63	0.84	4.64	0.94
19	4.21	0.91	3.72	0.87	4.47	0.99
20	4.17	0.88	3.73	0.85	4.48	1.00
21	4.20	0.90	3.80	0.84	4.37	1.00
22	4.30	0.91	3.88	0.86	4.31	1.00
23	4.38	0.96	3.92	0.84	4.22	1.02
24	4.35	0.92	3.96	0.84	4.25	1.01
25	4.41	0.98	3.97	0.84	4.24	1.03
26	4.46	1.00	4.02	1.00	4.16	1.07
27	4.52	0.97	4.07	0.80	4.09	1.04
28	4.57	1.00	4.05	0.82	4.05	1.04
29	4.54	0.98	4.06	0.84	4.04	1.05
30	4.61	0.99	4.07	0.84	4.02	1.03
31	4.62	1.01	4.08	0.80	3.98	1.07
32	4.58	1.00	3.97	0.80	4.07	1.04
33	4.65	1.08	3.98	0.83	4.00	1.09
34	4.72	1.07	4.12	0.88	3.86	1.10
35	4.78	1.00	4.11	0.82	3.81	1.00

Mean values. After examining the component ratings by the percentage technique, it will prove instructive to examine the mean values for each component year by year. This is a standard method of analyzing age changes by the sampling technique. The mean value of each component in each age group is given in table 2 and calculated lines of best fit are illustrated in figure 2. It is clear that all three trends are curvilinear and are fitted best by second-degree equation curves. The 1st and 3rd components seem to change in gentle curves with approximately opposite direction and slope. The curve of the 2nd component, muscularity, appears to be quite independent of the other two. It will be noted that low mean values for ages 32 and 33 give a downward trend to the 2nd component line in the 30's. This may well be a chance fluctuation due to the small numbers of men involved, and it is possible that the 2nd component plateaus or continues to climb in the third decade.

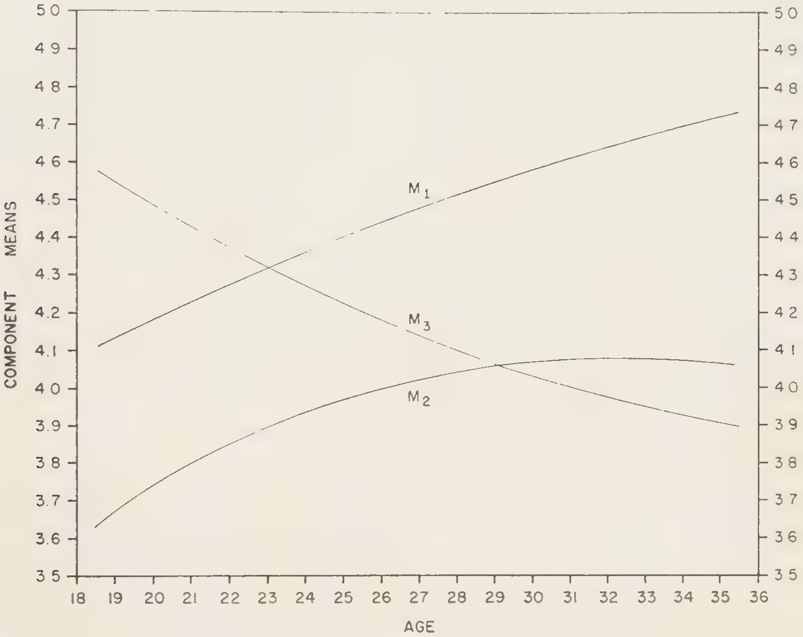

Fig. 2 Body build component mean values for ages 18 through 35.

It is interesting to follow the distribution formed by adding the mean values for each year. The significance of the sum of the component means is obscure biologically, but it is a statistically valid procedure under the circumstances. The line formed by successive summations is shown in figure 3. It is apparent that the summation value increases with age, due to the additive weight of the 1st and 2nd components and in spite of the 3rd component. Its curvilinearity appears due to the influence of rising muscularity. The slope of the summation values is probably the result of increased average weight with age, a well-known phenomenon. It has recently been shown that weight gains are due to an increase in both fatty and muscular tissue, in other words, the 1st and 2nd components (McCance and Widdowson, '51).

The evidence presented in figures 2 and 3 indicates that a change in body build occurs in the ages sampled. However, a word of caution is necessary before complete acceptance of these data. Firstly, it is not statistically proper to consider the components separately if they are mutually dependent. This will be investigated in later sections of the paper. Secondly, the data are derived from a military series at separation, and the influence of a military environment may somehow bias the results. The most obvious military bias would be the results of varying lengths of exposure to a rigorous environment. However, it is well known that all soldiers in the last war did not undergo a continuously strenuous existence. Hooton ('51) theorizes that the frequency of less rugged body builds may have been diminished through selective processes in military training with the older groups

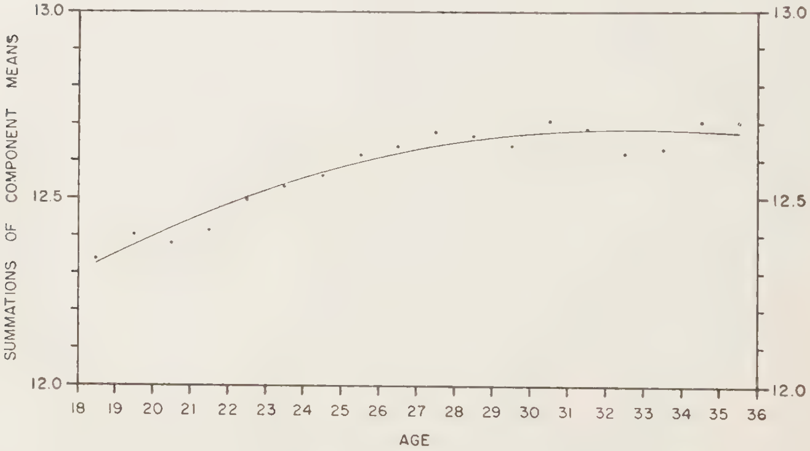

Fig. 3 Sums of component mean values for ages 18 through 35.

thus screened of weaker types. Unfortunately, there is no direct evidence for this hypothesis.

The average months of service at the time of separation for the White series analyzed here are shown in figure 4. Two lines of best fit based on second-degree equation curves have been plotted, since the data are not well-fitted by a single calculated third-degree equation curve, and the distribution indicates the presence of two distinct populations. The small group of 18-year-olds have not been included since these men were probably separated for reasons other than mere length of service. The marked bimodality indicated in figure 4 is evidently due to one or more sociological variables such as rank or overseas service. In any event there seems to be no direct and simple relationship between chronological age and length of military service in this body build sample.

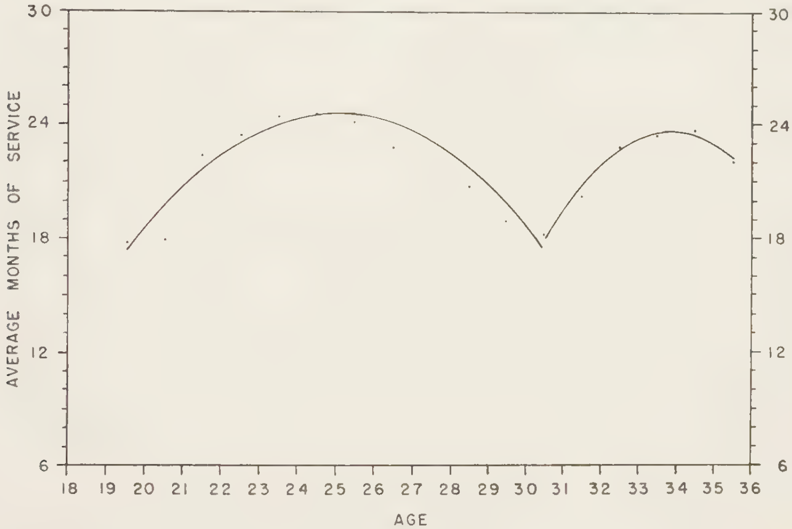

Fig. 4 Average months of military service for ages 18 through 35.

Not only is the effect of a military environment largely unknown, but every series, wherever sampled, has inherent in it certain sociological and probably biological biases. It would appear that a military series taken at a time when induction standards were remarkably low should not be less acceptable as a sample of the "general" population than a college series with its known age and economic restrictions.

Mutual dependence of the components. Product-moment correlation coefficients were calculated on the three possible interrelationships for each age group; 1st component versus 2nd component, 2nd component versus 3rd component, and 1st component versus 3rd component. For convenience these coefficients will be referred to as r_{12}, r_{23}, and r_{13}, respectively. The correlation coefficients have been plotted and lines of best fit drawn in figure 5. It will be noted that r_{12} is uniformly positive in value, while r_{23} and r_{13} are negative. The variability of the year values is indicated by the plotted points. Most of the variability can be ascribed only to chance variation and seems to be a function of the significance of the correlation coefficients, i.e., the vertical distance from a zero correlation. Some of the variability is undoubtedly caused by the action of the remaining component, and this will be handled later by the partial correlation technique.

The slopes of the lines in figure 5 are perhaps even more instructive for the purposes of this analysis than the actual coefficient values. The line r_{12} indicates a definite lowering of relationship with age illustrated by the differential slope of the mean value curves in figure 2.

The slope of line r_{23}, which differs in direction from the other two, seems to be due entirely to the indirect influence of the 1st component.

The line r_{13} shows only a slight change from 18 to 35, but the large size of the coefficients and the absence of variability make this change appear to have considerable significance. Its negative value is obvious since one cannot be fat and rotund and still score high in linearity.

Partial correlation. It is apparent from figure 5 that all of the components are somehow interrelated with each other in the total body build assessment. Fortunately, the analysis of partial correlation makes it possible to eliminate or hold constant the effect of the remaining component while examining each pair. The partial correlation coefficients for each age group are plotted in figure 6 with the number of the component held constant indicated behind the symbol “.”.

Several interesting facts emerge from this figure when compared to the preceding one. The line $r_{12.3}$ shifts from a low positive to an even lower negative value when the effect of the 3rd component is removed. By moving the line $r_{12.3}$ into a negative relationship position, the change with time has been reversed. The association increases negatively with the time whereas the r_{12} slope indicated an increasingly positive association. The most significant fact about the relationship between the 1st and 2nd components is surely their almost complete statistical independence.

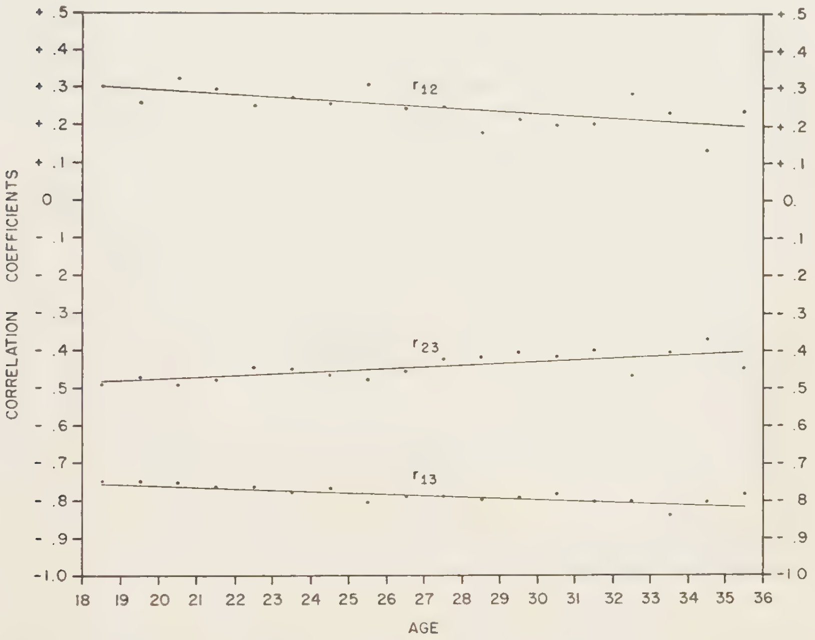

Fig. 5 Correlation coefficients between body build components for ages 18 through 35.

The line $r_{23.1}$ shows no differences for practical purposes between these components with age. It remains steadfastly in that difficult group of correlation values, too high to have no significance and too low to be of any great value for prediction.

The final line, $r_{13.2}$, shows little or no change from that plotted in figure 5. This is due to the minimal influence of the 2nd component on the 1st and probably to the uniform influence of the 2nd on the 3rd.

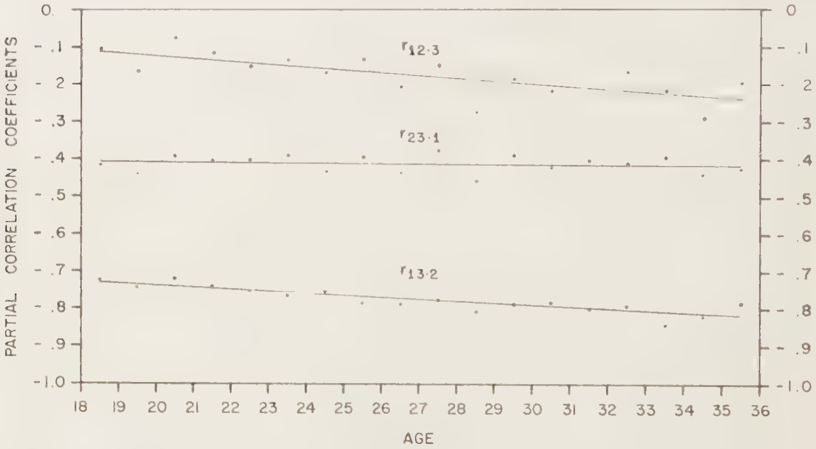

Fig. 6 Partial correlation coefficients between body build components for ages 18 through 35.

The data presented in figure 6 indicate a definite change in the relationships between the 1st and 2nd and 1st and 3rd components with time. It would argue against the immutability of body build even in the limited age range studied. However, the change is not great. The relative stability indicated by these data is, moreover, more properly ascribed to a statistical rather than a biological cause. This is due to the relatively coarse scale employed in any single component. All of mankind must be encompassed by only seven intervals of the 1st, 2nd, or 3rd component. If this range is to be covered, 1's and 7's must remain relatively scarce. This effectively crowds the majority of men into five or fewer categories. For example, in the total Army White series, 89.05% were rated as 3, 4, or 5 in the 1st component, 85.54% fell into the same scores in the 2nd component, and 86.14% received 3, 4, or 5 in the 3rd component. Thus about the only expected change under such a system of mensuration is that a high 4 may change to a low 5 or a low 4 to a high 3. If the total body build scale consisted of only three grades, 1's, 2's, and 3's, the somatotype would appear amazingly stable.

Comparison with other series. There are very few large published series available from which correlation coefficients may be derived for comparison. Sheldon, Stevens, and Tucker ('40) gave mean component values on 4000 men and correlation coefficients for a sample of 2000, presumably of these same men, from their original series, and Kraus ('51) provided the necessary data on 539 Japanese men so that correlation coefficients could be calculated. There are also two smaller racial samples available in the Army series, 3218 Negroid males and 492 Mongoloid males. In this comparison the total Army White series of 39,321 men was utilized. The mean values and standard deviations for each component and the correlation and partial correlation coefficients are given in table 3.

There are a number of limitations which must be kept in mind when comparing the values shown in table 3. As far as technique is concerned, Kraus states that he followed the system laid out by Sheldon, while the differences between the original Sheldon and Hooton techniques were outlined earlier in this paper. Thus, the higher 1st component values recorded by Hooton are probably due to his more liberal ratings. Conversely, the similar mean values for the 2nd component do not necessarily indicate equivalent muscularity since the Hooton system would score the same man lower in muscularity than the Sheldon rating. The 3rd component is arrived at by somewhat different methods and the means are probably not comparable. The age differences between the

TABLE 3

Comparison of equivalent values in different series

	1ST COMPONENT		2ND COMPONENT		3RD COMPONENT		CORRELATION COEFFICIENTS			PARTIAL CORRELATION COEFFICIENTS		
	Mean	S.D.	Mean	S.D.	Mean	S.D.	r_{12}	r_{23}	r_{13}	$r_{12 \cdot 3}$	$r_{23 \cdot 1}$	$r_{13 \cdot 2}$
Hooton (White)	4.34	0.95	3.87	0.86	4.29	1.04	+.30	—.47	—.78	—.12	—.40	—.76
Hooton (Negroid)	3.94	0.85	3.78	0.80	4.20	0.95	+.32	—.49	—.74	—.07	—.39	—.70
Hooton (Mongoloid)	4.05	0.84	3.89	0.72	4.15	0.97	+.38	—.50	—.80	—.04	—.36	—.76
Sheldon (White)	3.70	1.18	4.27	1.22	4.03	1.26	—.32	—.64	—.27	—.66	—.80	—.65
Kraus (Japanese)	3.54	0.68	5.97	0.75	1.88	0.57	+.13	—.59	—.43	—.17	—.60	—.44

groups must also be considered. The mean ages of the Army series are as follows: White, 23.9 years; Negroid, 23.2 years; and Mongoloid, 24.1 years. The mean ages are not available for the other two series, but 18 or 19 years seems reasonable for Sheldon's college men, and Kraus' Japanese must have averaged close to 30 years of age. It would seem reasonable to ascribe some of the differences seen in table 3 to this age factor.

Since some measure of the change in correlation due to age has been demonstrated in the Army series, it is possible to compare the coefficients on a more equitable basis. Actually, few if any of the discrepancies found in the correlation coefficients can be due to age differences, since the comparable age groups in the Army series are at least as far removed from the values based on Sheldon's and Kraus' data. Similarly, if the differences were due solely to moving the ratings in the 1st and 2nd components up and down one score, the derived relationships should show little change. The major identifiable source of discrepancy seems to be in the 3rd component. Removing the influence of this rating by means of partial correlation brings the coefficients closer together, but there still remain some unexplained differences. This is unfortunate since students of both schools will undoubtedly perpetuate and proliferate their respective techniques. Followers of Sheldon's methods will undoubtedly point out that differing degrees of ectomorphy can be associated with the same ponderal index according to his system. In any event the results should be made more comparable, if this were possible.

SUMMARY AND CONCLUSIONS

The problem of stability in body build has been investigated by examining a series of some 40,000 individuals photographed by the U. S. Army and typed at Harvard University. The age groups from 18 through 35 years have been analyzed for: percentage of occurrence for each rating, mean component scores, summation of component scores, product-moment correlation coefficients, and partial correlation coefficients on a year by year basis.

These analyses indicate that definite although limited changes are associated with age in this Army series. The 1st and 2nd components increase with age while the 3rd component decreases. The correlation between the 1st and 2nd components and 1st and 3rd components increase with age but the 2nd and 3rd components retain the same magnitude of association.

Similar values of central tendency and statistical association have been compared for White, Negroid, and Mongoloid series typed by the Hooton system and White and Mongoloid series typed by the Sheldon system. The values show large differences between investigators that pose problems of comparability in technique.

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A FACTORIAL STUDY OF CONSTITUTIONAL TYPE

W. W. HOWELLS
University of Wisconsin[1]

TWO FIGURES

INTRODUCTION

The study of the constitutional type of the individual was given a strong new impetus in 1940 by Sheldon. Although, in the form in which it travels most widely, and appears in the public prints, it looks like a system of types (so that a fat man may be referred to as an ''endomorph''), the essential novelty in it is the discernment of three ''components'' of constitution, which are thus by definition all necessarily manifest in the same individual, to differing degrees. Types, or discrete groups of individuals, are not the primary category of differentiation, but instead result from varying combinations of the strengths of the components.

This system, in its classical form, is classical also in its symmetry. For any individual, each component is rated on a 7-fold scale, by the eye of a trained inspector, so that the possible somatotypes resulting from these combinations may be laid out on a triangular map (Sheldon, '40, p. 118), with the types 7-1-1, 1-7-1, 1-1-7 at the corners and 4-4-4 in the middle; theoretically the distribution of individuals would form a three-sided pyramid over this base. There are two auxiliary concepts of importance. In the first place, the compo-

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nents themselves are negatively correlated: strength in one inhibits the others or, in plain language, a thin, linear man, high in ectomorphy, cannot be a fat man, high in endomorphy, nor a muscular, chunky one, high in mesomorphy. Sheldon's own rule of thumb is that in a valid somatotype the three component ratings cannot sum to more than 12 (Sheldon, pp. 61, 121). In the second place, any disharmony between different regions of the body in component ratings constitutes a "dysplasia" (literally a malformation, but in Sheldon's primary sense simply an irregularity).

It is obvious that somatotyping is an approach to important phenomena of physical differentiation,[2] and phenomena which have not been amenable to measurement or to other methods of physical anthropology, so that the system was warmly received by workers who had appreciated this lack, and by medical men as well. It is also apparent that different raters can attain a fairly good mutual uniformity in their ratings of a given individual, although this is not always the case. But there remain certain deficiencies in the development of the field, constituting serious limitations. Modifications of Sheldon's original system have been worked out by others, but there has been disappointingly little publication on such developments, or on general scrutiny of the potentialities or shortcomings of the whole, or of its implications.[3] In any case, there has been no essential alteration of Sheldon's basic procedure and it remains, at the moment, both in theory and in method, too much of an art and too little of a science. To become the latter, ways must be found of articulating it with other bodies of theory and lines of investigation. There are plenty of important questions waiting which are not answered by data obtained so far. For example, what is the essential nature of the components? What determines them:

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[2] Throughout this study every reference is to physique *only*, and the whole matter of physiological or temperamental correlates, postulated or demonstrated, is specifically ignored.

[3] This statement can be partly retracted in view of the articles by Hunt and Newman appearing in this issue of the Journal.

what is the genetic element, and what is the developmental latitude or determinant (Tanner, '47; Hunt, '49)? How far do some of the accepted phenomena grow out of the hypothesis itself? Are the three components actually equal partners, for the population? Is three the right number? Does their negative correlation imply some fault in the structure? Is the triangular map implicit in Sheldon's system the correct one, or is it too neat?

The need is probably for some satisfactory way of rendering constitutional data in metrical terms. That this would be a difficult task is obvious, from various studies of the anthropometric aspects of somatotypes already made. Sheldon himself provided ('40) metrical standards — tables for the determination of somatotype from measurements — but these were not put to other uses and have evidently been little employed for their original purpose. In the second place, workers in constitution are quite right in saying (Dupertuis, '50; Tanner, '51) that the anthroposcopic technique yields just what the anthropometric technique will not — an integrated appreciation of the individual, or data on muscle development vs. fat — and therefore in being impressed by the antithetical natures of the two methods. This does not, however, signify that some fresh means of quantification are not to be looked forward to in this field, such as will permit analysis on lines other than, but parallel to, the basically subjective procedure which is now the somatotyper's only recourse. It is hard to see how else it will be possible to obtain a critical evaluation, or some kind of fortification, of much of the work in constitution. It is to be hoped that such new methods will be forthcoming, particularly as a result of such improvements as the work of Tanner and others (Tanner, '51; Tanner and Weiner, '49; Dupertuis and Tanner, '50) in photogrammetric anthropometry.

Dupertuis attacked one aspect of the problem ('50) by metrically comparing three groups of men, of 10 each, characterized respectively by extreme endomorphy, mesomorphy and ectomorphy. He wished to discover what differences in conventional measurements might exist between constitutional extremes as represented by these three groups, and also which measurements might be shown to be useful by this method in discriminating between different somatotypes. He made comparisons in 33 measurements and 23 indices or ratios, of the traditional kind, as well as in the 17 measurement-stature ratios established by Sheldon, determined photogrammetrically. He found that the "nearest approach" (and certaintly not too near a one) to a useful anthropometry for somatotyping remains Sheldon's own set of measurements and that, while obvious and expected differences were found in girth measurements, nevertheless head and face measurements, and trunk and limb lengths, all failed to differentiate well between the groups. In other words, ordinary comparison of the means of typical body measures fails signally to distinguish these extreme groups (except as to bulk), and hence holds out very little significance or usefulness for comparable work with the more balanced and closely related somatotypes of the middle of the range. Dupertuis' careful work and report seem to leave little to be said in this particular matter.

Since an essential characteristic of the constitutional approach is its summing up of an individual (in contrast to the typical anthropometric summing up of a single isolated *trait* over a number of individuals), it has seemed to the writer that a parallel statistical approach, specifically the factorial method, might be used. This method consists simply in explaining a given set of available relationships (correlations) through a much-reduced second set of relationships (factors) derived from the first (see Howells, '51, for references). The writer was concerned with several of the questions posed earlier in this paper, and specifically with the question: Is the triangular map implicit in Sheldon's system the correct one? Doctor Dupertuis' material offered a chance to test this, and he very kindly furnished all of his original anthropometric data for the purpose.

A conventional trait analysis of measurements, particularly of so large a number, on such an oddly assorted group, might be instructive, but previous studies have indicated that a considerable number of factors would appear, almost certainly tending strongly to divide the body into regions, and that it might be difficult to interpret their bearing on the problem in hand. Instead, it was desired to invert the usual procedure, and correlate the *persons,* so that any given coefficient of correlation would express the total likeness or unlikeness, within the group, for two individuals. By this means it was thought that conceivably three distinct factors would emerge, directed respectively at clusters of persons representing endomorphy, mesomorphy and ectomorphy and thus, in a projection, actually reproducing geometrically the theoretical base triangle of somatotypes, possibly in a new form corresponding better to the ''true'' or natural relationships of the components. The actual results were quite otherwise, but no less interesting in the comments they offer.

It might be emphasized at the outset that this study has not been a search for metrical methods of distinguishing between somatotypes or components in their present accepted form, but rather an effort to find some basis for critical evaluation of somatotyping by using an independent approach. What follows may seem unduly complex, being cast in the language and methods of factor analysis. It is, however, not an attempt to create impressive complexity; it is nothing more than a formal way of finding all the mutual likeness and unlikeness of the individual subjects, arithmetically from their available measurements, and then attempting to explain the findings by the simplest, and smallest number of, kinds of difference; all this being derived from the material itself and not from *a priori* considerations.

MATERIAL AND METHOD

Out of his original 30 subjects, Dr. Dupertuis designated 15, comprising the 5 individuals most dominant of all, in his opinion, in each component. This reduction in number was for the sake of convenience, so as to have a 15 ×15 matrix in place of one 30 × 30, with the results expected to be essentially the same.

The total of measurements (by inversion, these become the "individuals" or number of cases in the computation) was 34, stature being added to the list in Dupertuis' paper. These, roughly grouped (as by Dupertuis, p. 371) to show that they offer some balance as to the aspect of body form measured, are:

> head length, head breadth, minimum frontal, bizygomatic, bigonial, face height, upper face height, nose height, nose breadth, interpupillary space,
>
> finger length, hand length, palm breadth, humerus length, radius length, total arm length, span, femur length, tibia length, total leg length, stature,
>
> sitting height, trunk height, biacromial, biiliac, chest breadth, chest depth,
>
> head circumference, neck circumference, chest circumference, upper arm circumference, lower arm circumference, mid-thigh circumference, calf circumference.

The next problem was to reduce all measurements to some kind of a common scale, since in getting the product-moment correlation between two individuals one is forced to add the products of the two nose heights to that of the two statures concerned, etc. The values might all have been reduced to standard form at once (expressed as deviations from the mean and divided by the standard deviation) but it was desired, in this case epecially, to avoid eliminating the relative range of different measurements at this stage. Accordingly, the mean of each measurement was found, and divided through the 15 individuals, so that each value was taken as a percentage of the mean for that measurement, and the mean of every measurement was automatically 100. (In the computation of *r,* of course, the mean becomes the mean of a *person,* that of all his measurements).

Table 1 gives the intercorrelations of the 15 individuals, N of course being 34. Numbers 1 to 5 are extreme endomorphs, 6 to 10 extreme mesomorphs, and 11 to 15 extreme ectomorphs.

FACTOR PATTERNS

Centroid (orthogonal) factors. After the reflection of the strongly negative members of the set, three factors were extracted by the centroid method. These and the communalities (h^2, the common factor variance) are shown in table 2, and the 3rd factor residuals appear in table 1. A 4th factor was attempted from these residuals, but there is every indication that it is not significant.[4]

This factor pattern says nothing, directly, about body form or measurements. It is purely and simply an arrangement of these particular 15 persons, in three different ways — along three independent, uncorrelated axes of differentiation which arise from the data, and which seem adequately to account for their individual likenesses and differences, insofar as the 34 measurements involved will convey them.[5] The pattern also measures each of the persons in terms of the three factors, whatever they are. The first factor, F_I, separates the 5 endomorphs (nos. 1–5) very definitely from the 5 ectomorphs and, as will be seen, no rotation will eliminate the bi-polarity of this factor, i.e. eliminate the negatives and do away with the opposition of these two groups. The conclusion is hardly to be avoided that this factor expresses the obvious main difference between the components involved,

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[4] From the small 4th factor values, from the 3rd factor residuals (table 1), from the contribution (.382, or 2.5%) of the 4th factor to the total variance (cf. table 2), and from attempts to rotate this factor with others.

[5] There can be no ''general'' factor in this arrangement, as there is in the typical trait analysis. Relative likeness between certain of the individuals demands relative unlikeness of these to others, so that there must be substantial negative correlations in the matrix R.

TABLE 1

R, matrix of correlations of persons, and 3rd factor residuals

	INDIVIDUAL NO.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
ENDOS	1	. . .	*—.034*	*—.005*	*—.011*	*—.014*	*—.015*	*+.047*	*+.041*	*—.054*	*+.018*	*—.006*	*+.024*	*.000*	*+.022*	*+.008*
	2	.788	. . .	*—.005*	*+.020*	*—.002*	*—.017*	*—.024*	*+.045*	*+.033*	*—.016*	*—.057*	*—.018*	*+.023*	*+.031*	*+.043*
	3	.900	.763	. . .	*—.010*	*—.054*	*—.010*	*+.045*	*—.067*	*+.029*	*+.018*	*+.013*	*+.012*	*—.009*	*+.030*	*+.012*
	4	.689	.575	.753	. . .	*+.030*	*+.037*	*—.018*	*+.003*	*+.026*	*+.074*	*+.022*	*+.007*	*—.019*	*—.040*	*—.054*
	5	.746	.571	.689	.416	. . .	*—.062*	*—.045*	*+.019*	*+.037*	*+.071*	*+.024*	*+.030*	*+.031*	*—.078*	*.000*
MESOS	6	—.364	—.389	—.417	—.582	—.145	. . .	*+.124*	*—.003*	*—.052*	*—.005*	*+.016*	*—.029*	*+.002*	*+.098*	*+.040*
	7	—.121	—.222	.032	.176	.098	—.224	. . .	*+.031*	*—.007*	*—.087*	*—.002*	*+.035*	*+.002*	*+.020*	*+.007*
	8	—.039	—.218	.169	.101	.156	.039	.497	. . .	*—.103*	*+.020*	*—.021*	*+.016*	*—.058*	*+.028*	*—.045*
	9	.513	.221	.512	.472	.476	—.119	.387	.257	. . .	*+.026*	*+.020*	*—.052*	*+.037*	*—.091*	*+.036*
	10	.221	—.026	.218	—.153	.565	.332	.274	.305	.329	. . .	*—.028*	*+.025*	*—.004*	*+.015*	*+.001*
ECTOS	11	—.879	—.745	—.903	—.761	—.751	.388	—.207	—.196	—.649	—.325	. . .	*+.020*	*+.034*	*—.016*	*+.017*
	12	—.909	—.683	—.920	—.679	—.838	.339	—.111	—.177	—.659	—.376	.912	. . .	*—.003*	*—.015*	*—.040*
	13	—.887	—.742	—.878	—.655	—.842	.263	—.061	—.198	—.518	—.357	.862	.907	. . .	*—.022*	*—.022*
	14	—.876	—.692	—.876	—.605	—.760	.080	—.065	—.156	—.666	—.475	.894	.885	.873	. . .	*+.027*
	15	—.768	—.557	—.827	—.642	—.802	.153	—.308	—.397	—.645	—.510	.873	.849	.833	.904	. . .

which is mass, or globularity, or fat, and one may accordingly guess that no. 6, though a dominant mesomorph, is more linear than globular, and that no. 9 is the opposite. Factor F_{II} in general gathers in the mesomorphs and opposes them to the endomorphs. F_{III} divides the mesomorphs into two groups, nos. 6 and 10 vs. 7 (particularly), 8 and 9.

TABLE 2

Orthogonal (centroid) factor matrix, F_0

INDIVIDUAL		FACTOR			COMMUNALITY
		I	II	III	h^2
ENDOS	1	.877	.334	−.220	.929
	2	.665	.555	−.244	.810
	3	.923	.259	−.040	.921
	4	.721	.355	.332	.756
	5	.817	−.091	−.334	.787
MESOS	6	−.329	−.465	−.431	.510
	7	.245	−.488	.571	.624
	8	.265	−.511	.266	.402
	9	.669	−.266	.175	.549
	10	.378	−.637	−.380	.693
ECTOS	11	−.968	−.091	−.028	.946
	12	−.959	−.073	.092	.933
	13	−.923	−.101	.199	.902
	14	−.916	.022	.263	.909
	15	−.941	.218	.038	.934
Contribution to total variance (= 15.0)		8.513	1.884	1.208	11.605
Percentage of total variance		56.8%	12.6%	8.1%	

It is possible to show the relations to the individuals graphically, in a space of three dimensions, since there happen to be only three factors. This is done in figure 1. A sphere is used, for visual convenience, and the individuals are shown as vectors from the center extending to the surface of the sphere, through which the axes of the factors, F_I, F_{II} and F_{III} also pass at right angles to one another (being uncorrelated).[6] This makes it possible to comprehend the information of table 2 all together. The endomorphs, 1 to 5, are ranged around the north, or positive, pole of F_I, while the ectomorphs are tightly clustered around the south, or negative pole. The mesomorphs are spread out on one hemisphere, with 6 and

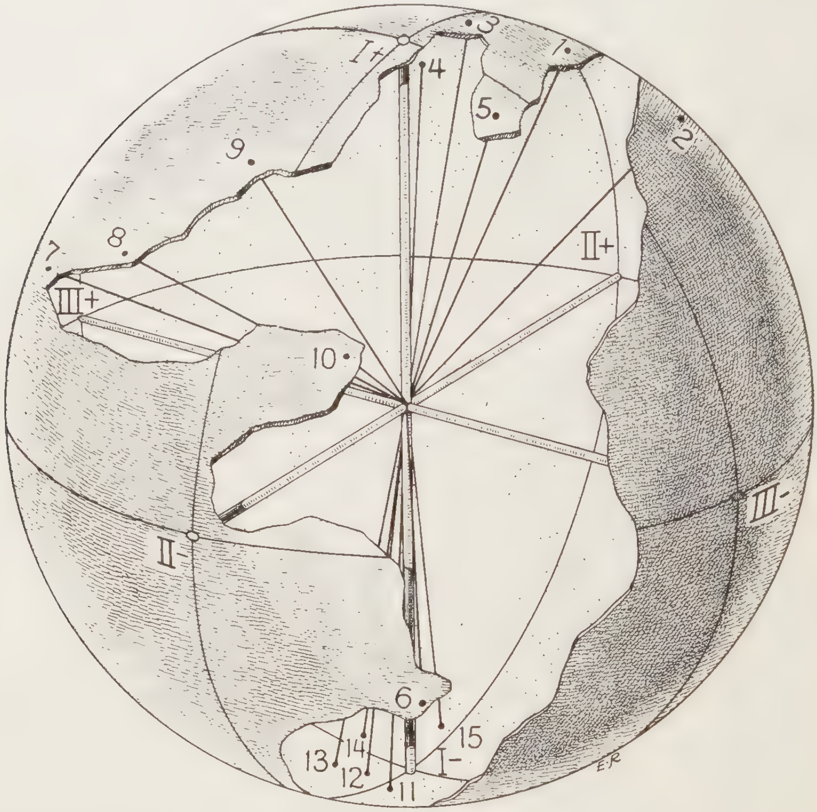

Fig. 1 Factorial relationships of the 15 individuals to one another and to the axes of the three F_0 orthogonal factors. As a visual aid, the vectors of the individuals are shown as though projected to an enclosing sphere. (This corresponds to the factorial structure, consisting of the fixed configuration of the individual vectors and the movable axes of the factors, which are here in the position of the F_0 pattern.)

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[6] This was not prepared throughout by exact calculation, but simply by plotting the extended vectors on planes (see Thurstone, '47, ch. xi) and using these as indicators to place the vectors as shown.

10 in one quadrant and 7, 8 and 9 in another, 7 being in the extreme position and 9 being not too far removed from the endomorphs. Now, leaving aside the significance of these last relationships for the moment, one fact apparent from the factor pattern, F_0, is strongly stressed in figure 1: that endomorphy and ectomorphy, *as far as the 34 measurements used herein will register them,* are the same thing, the same component, being simply the positive and negative halves of the same scale.

Oblique factors. The question next arises as to whether the centroid factor pattern can be improved, so as to show the relations of the individuals more clearly, or make more suggestions as to the nature of the differentiations among them. Figure 1 suggests that F_I is placed as well as possible, but that the other two might be moved, so that one is midway between no. 6 and no. 10, to register whatever their special status may be, and the other placed so that it coincides with no. 7. This is easily done, by Harris' method ('48; see Howells, '51, p. 169 for summary). Plots of the results indicate adjustments in the rotations to sharpen the results, by increasing apparently significant loadings and reducing apparently insignificant ones — in other words, placing these axes in truer positions for the whole system, judging from the figures.

The result appears in table 3, in the form of a simple-axis solution, V. The transformation matrix, Λ, is also given, and the intercorrelations, Φ, between these now oblique, correlated factors. It is to be seen that the first factor, V_I, is still practically uncorrelated with the others, while V_{II} and V_{III} have an r of .420, which seems to recognize their essential connection with mesomorphy, or at least within the mesomorphs of this particular group. As to the factor pattern itself, the contrast is enhanced between nos. 6 and 10 on the one hand and nos. 7, 8 and 9 on the other, but no fresh information seems to have been developed. This version therefore seems merely to affirm the acceptability of the centroid solution.

Orthogonal rotation. Because of the unsatisfactoriness of the oblique solution, a simple orthogonal rotation was tried; this one to improve the first orthogonal solution as far as seems possible, while keeping the factors definitely uncorrelated and independent which, in the present case, with nothing known so far about the nature of the factors, seems desirable. The rotation is done, not by reference to figure 1, but by plotting the factors two at a time and looking for improved positions of the axes relative to the persons (or, if you like, taking any of the three equatorial sections of fig. 1 and revolving it as desired in the same plane, around its own center, so that all three axes remain at right angles). This means that the axes change position while the vectors of the individuals remain fixed. The final result of three such rotations appears as F_A, in table 4. This pattern does seem to be an improvement over both F_O and V. A large number of loadings are reduced to negligible size, or nearly zero, while at the same time those which seem to make significant

TABLE 3

Oblique factor pattern, V, with transformation matrix and correlations of factors

INDIVIDUAL	FACTOR I	FACTOR II	FACTOR III
1	.954	—.193	.036
2	.804	—.381	—.048
3	.941	.002	—.088
4	.684	.202	—.461
5	.834	—.051	.337
6	—.311	—.140	.600
7	.000	.782	—.271
8	.084	.557	.008
9	.541	.424	—.028
10	.313	.139	.637
11	—.933	—.162	.068
12	—.948	—.075	—.046
13	—.944	.033	—.127
14	—.927	.015	—.242
15	—.858	—.281	—.137

TRANSFORMATION MATRIX, Λ (FΛ = V)

I	II	III
.951	.198	.000
.209	—.575	—.474
—.229	.794	—.881

INTERCORRELATIONS OF FACTORS, Φ

	I	II	III
I	1.000		
II	.078	1.000	
III	—.060	.420	1.000

distinctions tend to be increased. (Factor II becomes reversed in sign, but this has no effect, and is left thus as being more suitable).[7]

Taken all together, the three patterns, based on three somewhat different sets of reference axes, indicate that the nature of the factors is relatively definite, and that the orthogonal rotation achieved in F_A is approximately the best solution attainable. The net result is that the shape relationships of the 15 individuals are shown to be describable by three distinct factors, or components in this system, which actually give their clearest result when entirely uncorrelated and independent of one another. Of the three factors, *not more than*

TABLE 4

Orthogonal (rotated) factor matrix, F_A

INDIVIDUAL	FACTOR		
	I	II	III
1	.935	—.232	—.021
2	.759	—.483	—.005
3	.944	—.105	.134
4	.703	—.133	.493
5	.839	.133	—.256
6	—.318	.275	—.577
7	.093	.653	.433
8	.155	.599	.139
9	.596	.408	.164
10	.346	.569	—.500
11	—.957	—.080	—.157
12	—.963	—.064	—.036
13	—.948	—.002	.062
14	—.935	—.102	.158
15	—.901	—.351	.002
Contribution to total variance	8.576	1.829	1.198

---

[7] For the geometrically-inclined: with reference to figure 1, in the new configuration F_A factor I has been rocked somewhat so that it lies between 1 and 3 above and close to 13 below; II and III have revolved clockwise (and II has changed ends, or signs) so that II is near the longitude of 7, and III near to that of 6 and 10 at its minus end and to 4 at the plus end.

one can be clearly associated with the kind of differentiation represented in Sheldon's system by both ectomorphy and endomorphy. The specific nature of the factors, particularly II and III, cannot be told from this much of the analysis, and a further step is called for.

INTERPRETATION

Factors and measurements. An ordinary factor analysis gives a factor pattern, consisting of a set of linear equations describing each trait or *variable* in terms of the *factors.* It is possible then to derive also a set of equations describing each *factor* in terms of the *variables,* and thus to be able to score an individual in factors, from his known scores in the variables. In the present analysis, with its inverted approach, this means that we have already seen a pattern describing each *person* in terms of *factors* representing their three kinds of mutual arrangement. We now wish a pattern, or set of equations, describing each *factor* as it is composed of *persons.* Having this, we can then score each of the original 34 measurements on the factors by inserting the values for the individuals in these new equations. Thus we arrive at an estimate of which measurements (not which individuals) express each factor most strongly.

The matrix of these equations is given (transposed) in table 5.[8] The scores of the measurements, to be multiplied by the coefficients in table 5, are given in table 6; these have been standardized from the scores originally used for the correlations by getting a mean for each individual, and expressing each of his measurements as a deviation from that mean, divided by the standard deviation for the individual. (This has the effect of eliminating the absolute size of a person and expressing him purely in terms of shape; i.e. each of his measurements is now a sort of index or ratio based on all his measurements together.)

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[8] The computation is described by Holzinger and Harman ('41, pp. 278–282), and by Thomson ('51, ch. xv). In the present instance, the matrix of table 5 is obtained by

$L^{-1}F'(U^2)^{-1}$,

where $F =$ orthogonal factor matrix (here F_A)

$(U^2)^{-1} =$ diagonal matrix of reciprocals of the unique factors, i.e. $1/(1\text{-}h^2)$

$L = I + J$

$I =$ the identity matrix (ones in the diagonals, zeros elsewhere).

$J = F'(U^2)^{-1}F$

Nature of factors. The final result, the scores of the measurements on each factor in the F_A pattern, is given in table 7. Instead of simply being tabulated, the measurements have been ordered in each factor according to the scores obtained, and this gives an automatic description of the nature of the factor. All of them, of course, are bi-polar, segregating the individuals in two directions.

TABLE 5

Equations for scoring measurements on F_A factors (read vertically)

	I	II	III
1	.157	—.447	—.193
2	.055	—.317	—.073
3	.122	—.183	.221
4	.021	—.044	.355
5	.047	.034	—.243
6	—.002	.049	—.210
7	—.013	.221	.246
8	—.004	.120	.059
9	.007	.106	.076
10	.013	.185	—.286
11	—.167	—.139	—.473
12	—.146	—.051	—.026
13	—.109	.056	.184
14	—.117	—.053	.381
15	—.122	—.566	.004

Factor I: As has been taken for granted, this contrasts girths with lengths or, roughly speaking, endomorphy with ectomorphy. Individuals positive in this factor are not necessarily short, or short-limbed, but rather girth, not length, is the prominent aspect of physique; and it is above all fullness of the upper arm and thigh which marks them: there is a taper effect to the fleshy limbs. At the opposite extreme,

TABLE 6

"Person scores" of the measurements. (Measurements in each column expressed as deviations — in standard deviation units — from mean of each column, or person)

	ENDOS					MESOS					ECTOS				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Head length	— .42	— .72	— .06	— .49	— 1.49	.37	.82	.70	.13	— .06	.20	.67	.91	— .10	— .03
Head breadth	— .59	— .26	— .06	.53	.01	— .76	.37	1.45	— 1.39	.35	— .10	.45	.13	.65	— .15
Minimum frontal	— .50	— .37	.30	.53	— .29	— 1.71	1.28	.70	— .06	— .47	.00	.13	.24	.33	.20
Bizygomatic	— .59	— .14	.21	.82	— .44	— .76	1.28	1.20	— .63	— .06	— .10	.45	.01	.22	— .27
Bigonial	— .02	— .37	— .34	— .20	.61	— .95	— .09	.70	.71	— .67	.00	.02	— .55	.65	.56
Face height	— .34	— 1.06	— .52	— .20	.76	.56	.60	.70	1.09	.15	.10	.13	.80	— .10	— .97
Upper face ht.	— .42	— 1.41	— .98	.09	— .14	.75	.14	— .80	1.28	— .06	.70	.24	.69	.44	.44
Nose height	— .83	— .95	— .61	.67	— .44	— .01	1.28	— .30	— .25	— .67	.10	.13	.57	.87	.91
Nose breadth	— .83	— .14	— .43	.09	.16	2.64	.60	.95	.71	1.17	— .20	.56	.01	— .95	— 1.21
Interpupillary	— .50	— .37	.12	.53	— .59	— .19	— .09	1.45	.13	— 1.29	.20	— .41	.46	.65	.32
Finger length	— .59	— 1.06	— 1.25	— .78	— .29	.56	— .09	— .55	— .44	1.17	1.19	.88	.24	.44	.44
Hand length	— .34	— .60	— .80	— 1.07	.61	— .01	.82	— 1.80	— .82	.76	.50	.56	.57	.76	.44
Palm breadth	— .02	— .72	— .25	— 1.36	.61	.37	1.28	.95	— .06	1.58	.30	— .30	.01	— .10	— .39
Humerus length	— .91	— .26	— 1.44	— .92	— 1.49	.37	— .77	.20	— 1.39	— 1.70	1.19	1.85	1.13	1.19	1.38
Radius length	— .83	.08	— .89	— 1.79	— .89	.56	— 1.45	— 1.80	— .63	— .06	1.29	.77	1.13	.87	1.49
Total arm length	— .99	— .37	— 1.16	— 1.21	— .59	1.88	— 1.22	— 1.05	— 1.01	.26	.90	1.10	1.02	.87	1.02
Span	— .67	— .14	— .89	— .92	— .59	.56	— .77	— .55	— .44	— .06	.60	.77	.80	.65	.79
Femur length	— 1.15	— .14	— 1.07	— .92	— 1.04	— .01	— .77	— .80	— 1.01	— .67	1.00	1.21	1.47	1.29	1.14
Tibia length	— .50	— .95	— .52	— 1.36	— 1.34	2.26	— 2.36	— 1.05	— 1.77	— .47	1.79	.99	.69	.54	1.49
Total leg length	— .75	— .72	— 1.16	— .92	— .44	.37	— 1.22	— .55	— 1.20	— .67	1.39	1.10	.69	1.08	1.49
Sitting height	— .42	— .37	— .25	— .20	— .59	— .95	.60	— .05	— .25	— .06	.30	.24	.69	.54	.44
Trunk height	— .42	— .26	— .06	— .20	.01	— 1.14	.37	— .05	.33	.35	.10	— .09	.24	.76	.32
Biacromial	— .26	.66	— .06	— .63	— .89	.37	1.05	.45	1.09	— .47	— .39	.24	— .21	— .20	.32
Biiliac	1.77	2.84	1.03	1.69	.91	— 1.52	— 1.68	— 1.55	— 1.77	— 2.11	— .79	— .74	— 1.44	— .31	— .50
Chest breadth	.79	— .03	1.12	2.27	— .44	— .76	.60	— .05	1.28	— 1.08	— .49	— .63	— 1.11	— .63	— .86
Chest depth	2.34	1.35	1.40	2.13	.01	— 1.52	— 1.22	— 1.55	1.85	— .26	— 2.08	— 1.38	— .55	— 1.70	— .62
Head circum.	— .50	— .49	— .16	— .05	— .89	— .19	.82	.70	— .44	.15	.10	.45	.57	.44	— .03
Neck circum.	.71	1.00	.57	.53	.91	— .57	1.28	.20	.71	.76	— .69	— .95	— .99	— .95	— .97
Chest circum.	1.77	1.81	1.67	1.55	1.21	— 1.14	.82	— .55	1.47	— .88	— 1.59	— 1.71	— 1.44	— 1.49	— 1.44
Upper arm circ.	2.34	1.46	1.85	.67	2.86	.75	.37	.70	1.09	3.01	— 2.68	— 2.46	— 2.67	— 2.56	— 2.27
Lower arm circ.	1.04	— .14	.94	— .05	1.66	.94	— .09	2.70	1.47	1.58	— .79	— 1.60	— 1.67	— 1.28	— 1.56
Mid-thigh circ.	1.53	2.61	2.22	.97	2.11	— .38	— 1.00	— 1.05	.90	.76	— 1.69	— 1.92	— 2.00	— 2.24	— 1.44
Calf circum.	1.69	.77	2.22	.67	1.06	— .38	— 1.22	.70	— .06	.56	— 1.19	— 1.38	— 1.11	— 1.38	— 1.44
Stature	— .59	— .49	— .70	— .49	— .59	— .38	— .31	— .30	— .63	— .26	.80	.67	.69	.76	1.02

TABLE 7

Scores of measurements on F_A factors[1]

I		II		III	
2.53	Upper arm circ.	1.90	Nose breadth	1.52	Chest breadth
2.03	Mid-thigh circ.	1.58	Face height	1.46	Minimum frontal
1.68	Chest circum.	1.31	Lower arm circ.	1.30	Nose height
1.58	Chest depth	1.26	Palm breadth	1.21	Interpupillary
1.52	Calf circum.	.85	Upper arm circ.	1.20	Bizygomatic
1.25	Lower arm circ.	.70	Head length	.92	Chest depth
1.12	Biiliac	.66	Bizygomatic	.88	Chest circum.
.88	Neck circum.	.61	Head circum.	.74	Head breadth
.76	Chest breadth	.56	Upper face ht.	.72	Head circum.
		.52	Head breadth	.65	Sitting height
.03	Nose breadth	.50	Finger length	.56	Trunk height
— .02	Palm breadth	.45	Neck circum.		
— .04	Biacromial	.29	Nose height	.52	Head length
— .13	Bigonial	.24	Minimum frontal	.47	Biacromial
— .14	Face height			.35	Biiliac
— .15	Bizygomatic	.22	Hand length	.35	Bigonial
— .20	Minimum frontal	.19	Trunk height	.09	Humerus length
— .24	Head breadth	.11	Biacromial	.06	Femur length
— .24	Trunk height	.09	Sitting height	.00	Stature
— .26	Interpupillary	.00	Interpupillary	— .03	Neck circum.
— .37	Head circum.	— .08	Chest breadth	— .06	Face height
— .42	Head length	— .20	Bigonial	— .06	Upper face ht.
— .43	Sitting height	— .30	Span		
— .55	Hand length	— .40	Total arm length	— .39	Calf circum.
— .59	Upper face ht.	— .41	Stature	— .50	Span
— .60	Nose height	— .44	Calf circum.	— .63	Hand length
		— .52	Chest circum.	— .73	Total leg length
— .73	Span			— .85	Nose breadth
— .76	Stature	— .57	Femur length	— .92	Mid-thigh circ.
— .78	Finger length	— .86	Mid-thigh circ.	— .96	Palm breadth
— 1.01	Total arm length	— .88	Total leg length	— 1.01	Total arm length
— 1.03	Radius length	— .89	Humerus length	— 1.16	Radius length
— 1.04	Tibia length	— 1.08	Radius length	— 1.19	Lower arm circ.
— 1.12	Total leg length	— 1.31	Tibia length	— 1.20	Finger length
— 1.18	Femur length	— 1.44	Chest depth	— 1.55	Upper arm circ.
— 1.35	Humerus length	— 2.77	Biiliac	— 1.76	Tibia length

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[1] The spacings in the columns have been placed so as to make suggestive groupings of high, neutral and low, and are without other significance.

individuals negative on the factor are markedly skinny: limb lengths are prominent in their appearance, and especially the length of the first segments, upper arm and thigh. Both limbs and chest are poor in girth. There is a suggestion of greater prominence of facial features in these, but on the whole features of the head and face are little involved in this factor.

Factor II: This is more peculiar: it gathers the head and arms together and opposes them to the trunk and legs; perhaps more specifically it opposes the head to the rest of the body, but also the arm circumferences to those of the leg. Positive individuals should be marked by general top-heaviness: large and rugged heads and well developed but relatively *short* arms (not resembling ''endomorphic'' arms, since the forearm girth is particularly prominent); small legs, rather shallow (not necessarily small) chests and extremely slender hips, in the relative sense. This suggests altogether a sort of professional strong man, but apparently does not constitute simply hypertrophy of the arm muscles since it mainly emphasizes the head. The opposite in this factor is relatively stronger in his underpinning and hips, with a deeper chest; he would have lighter and relatively longer arms and a generally smaller head and, particularly, a narrow nose. It is not easy to picture this general configuration.

Factor III: This opposes the trunk to the limbs, and also associates a blossoming of the midface with bodily chunkiness. The positive end of the factor is marked by fullness of chest and somewhat less by emphasis on sitting height and trunk height, also by breadth across the upper face and by nasal height; the limbs on the whole appear relatively small, and the lower segments *(not* the upper) appear short. Possibly the typical television wrestler would be high on this factor. Individuals at the other extreme would have opposite tendencies, with relatively long, full limbs, and especially well filled out (muscular?) arms; the trunk would be relatively smaller and slighter, and the head also not heavy in construction, with a more constricted and delicate face. The image suggested is more of a welterweight boxer than of a wrestler. (Of course there is no real suggestion of athletic endowment connected with ratings of these factors.)

An attempt has been made to present the above distinctions pictorially in figure 2, in which two idealized individuals have been sketched to contrast the plus and minus ends of the scale for each factor, as though that factor alone were concerned in the form of these individuals.

Lest it seem that there is some overlap between the factors, particularly factor II and the others, it should be remembered that all of them are specifically uncorrelated, an important fact. Altogether, it appears that factor I provides a crude basic scale for endo-ectomorphy, and that the mesomorphs (see nos. 6–10 in table 6) are all definitely positive on factor II, which most of the remainder are not.

Reality of factors. A question arises as to the soundness of the interpretation provided by the above treatment: it should be remembered, with reference to figure 1, that the arrangement of the 15 persons (as shown by their vectors) is fixed, and the number of independent factors is established at three, but that the reference axes for these factors might be placed arbitrarily in an infinite number of positions. The three patterns, F_O, F_A and V, represent three good positions, with F_A indicated as approximating the most logical of all. Now, the apparent nature of the factors will depend on the placement of these reference axes among the individuals. Will other solutions than the one used give similar or reconcilable results, or are the results above largely random in nature?

The same routine was repeated with the F_O factor pattern, the unrotated centroid axes. Without reproducing the results, it may be said that they were very much the same, but less well defined. Finally, the same was done with the oblique factors, V (see Holzinger and Harman, '41, for computation). These new results, for the measurements, are shown in table 8. It is to be seen that there is a strong correspondence with the F_A results in factors I and II, but that there is some divergence between the two versions of factor III,

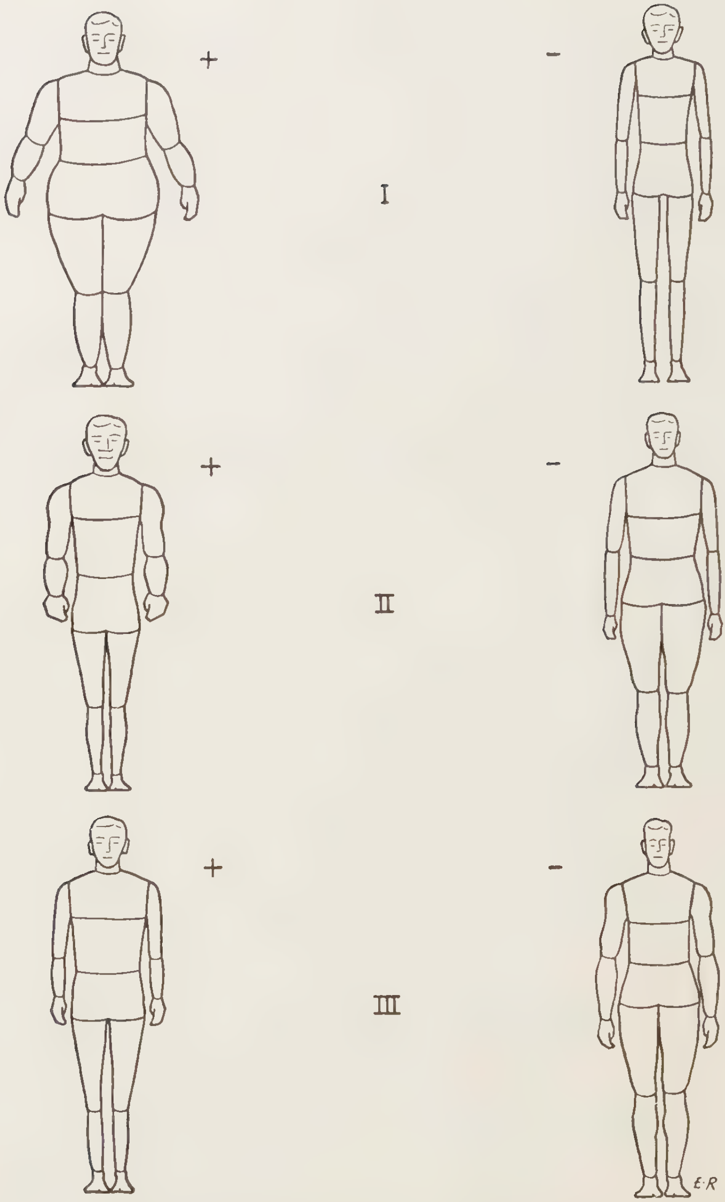

Fig. 2 Diagrammatic representations of distinctions in body form corresponding to F_A factors I, II and III (see table 7 and text). These have been done freehand, from the information in table 7; precise proportions are not afforded by the data. Furthermore, they do not represent types, but rather bi-polar tendencies, or "components," which may all be expressed in any one individual.

TABLE 8

Scores of measurements on V oblique factors

I		II		III[1]	
2.39	Upper arm circ.	1.66	Nose breadth	1.83	Nose breadth
2.01	Mid-thigh circ.	1.51	Face height	1.82	Upper arm circ.
1.76	Chest circum.	1.15	Lower arm circ.	1.77	Lower arm circ.
1.62	Chest depth	1.01	Palm breadth	1.54	Palm breadth
1.52	Calf circum.	.90	Bizygomatic	1.24	Finger length
1.28	Biiliac	.78	Upper arm circ.	1.01	Face height
1.12	Lower arm circ.	.74	Head length	.63	Hand length
.85	Neck circum.	.71	Head circum.	.55	Tibia length
.84	Chest breadth	.64	Head breadth	.54	Total arm length
		.62	Minimum frontal	.38	Upper face ht.
— .02	Biacromial	.53	Neck circum.	.32	Neck circum.
— .10	Bigonial	.50	Nose height	.23	Radius length
— .11	Bizygomatic	.46	Upper face ht.	.23	Mid-thigh circ.
— .12	Nose breadth			.20	Span
— .12	Minimum frontal	.35	Chest breadth	.06	Calf circum.
— .14	Palm breadth	.28	Trunk height		
— .19	Interpupillary	.24	Interpupillary	.01	Total leg length
— .22	Head breadth	.21	Biacromial	.01	Head length
— .22	Face height	.18	Sitting height	— .20	Head circum.
— .22	Trunk height	.12	Finger length	— .27	Stature
— .36	Head circum.	.01	Hand length	— .27	Head breadth
— .40	Sitting height	— .12	Chest circum.		
— .42	Head length	— .12	Bigonial	— .30	Biacromial
— .54	Nose height	— .35	Calf circum.	— .32	Trunk height
— .59	Hand length	— .49	Span	— .39	Bigonial
— .62	Upper face ht.	— .49	Stature	— .42	Femur length
				— .47	Sitting height
— .74	Stature	— .68	Femur length	— .55	Bizygomatic
— .75	Span	— .73	Total arm length	— .65	Humerus length
— .87	Finger length	— .81	Mid-thigh circum.	— .85	Nose height
—1.05	Radius length	— .99	Humerus length	— .96	Minimum frontal
—1.05	Total arm length	—1.01	Chest depth	— .96	Interpupillary
—1.07	Tibia length	—1.14	Total leg length	— .99	Chest circum.
—1.11	Total leg length	—1.42	Radius length	—1.24	Chest breadth
—1.15	Femur length	—1.78	Tibia length	—1.57	Chest depth
—1.29	Humerus length	—2.48	Biiliac	—1.96	Biiliac

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[1] As in the case of the factor loadings themselves (tables 3 and 4), the signs for factor III re reversed as between the values here and for the F_A factors, table 7. This is of no signifiance; but the arrangement is kept here, for ease of comparison between factors II and III, hile in comparing table 8 with table 7, the column for factor III should be reversed.

V_{III} converging on the nature of factor II, of table 7, especially in the placement of nose breadth on one hand and biiliac and chest depth on the other.

Now, the oblique factors V_{II} and V_{III}, unlike those of the F_A pattern, are *not* uncorrelated, and in fact have a correlation of .420, having been placed with reference to two groups of the mesomorphs, converging somewhat on one another. This seems not only to explain the likeness in results in scoring the measurements on V_{II} and V_{III}, but also to suggest that their common features are particularly significant for mesomorphy (at least within this group): pronounced relative nasal breadth, forearm and upper arm circumference, palm breadth and face height, and relatively markedly narrow hips and shallow chests. And, in fact, reference to the "standardized person" scores (table 6) shows that the 5 mesomorphs do tend consistently to show these same kinds of deviation from their individual personal "average" measurements.

Comparison with original material. Given three factors, or metrically found components, the nature of these components, and the rating of specific individuals on them, it should be possible to diagnose the appearance of the individuals, sight unseen, to a considerable degree, within the obvious limits of the data. The writer attempted just that (he has not seen photographs of the subjects, or any non-metrical data beyond their somatotype grouping), and submitted the results to Dr. Dupertuis who with great kindness checked them over from his own data on the men. (A first, less successful attempt was made from the original centroid pattern, before making the analysis of the factors themselves.) This showed that it was possible to arrive at a fairly specific description, within expected limits, and in fact no humiliating defeats were met with. Space cannot be given to a full account of this check, but the following are representative cases, with results.

Individual no. 2. Rating on F_{II} (from table 4): — .483. Diagnosis: "Endo, with more pronounced tendency (than no. 1) to relatively small head, wide hips, relatively large legs *(not so* according to your previous notes).'' Check: ''Does have tendency to relatively small head but especially short face it seems to me. Yes, has extremely wide hips and I would say that his thighs are quite large but not his lower legs. His thighs are also long. He appears to be relatively weak from the knees down.''

Individual no. 3. Rating on F_I: .944, on others nothing pronounced. Diagnosis: ''The type endo.'' Check: ''Yes, about as good a type endo as we have.''

Individual no. 4. F_I: .703; F_{III}: .493. Diagnosis: ''Endo, less marked than others; relatively large trunk and midface, short limbs.'' Check: ''This man does appear to have relatively long trunk and quite wide and fairly deep chest. His legs are slightly short but not nearly so short as his arms. His upper face and nose are large.''

Individual no. 7. F_{III}: .433; F_{II}: .653. Diagnosis: ''Relatively very long in trunk, broad in midface; excessively top-heavy.'' Check: ''Yes, extremely long trunk and top heavy. Has broad shoulders relative to biiliac. Huge development of muscles in upper trunk, especially pectoralis. Does have wide face and forehead on medium wide head.''

Individual no. 9. F_I: .596; F_{II}: .408. Diagnosis: ''Most endo (or well-padded?) of the mesos; perhaps combination of this, with marked head and arm development, gives a picture-book meso.'' Check: ''I wouldn't say that this man is the most endo of the mesos but he certainly is the most well padded, with muscles. He tapers off a little in his wrists and hands and in his lower legs and feet. He is the comic strip idea of the true muscleman, the Superman, Lil' Abner, etc. He has quite long face and broad lower jaws. His arm development is terrific.''

Individual no. 12. F_I: —.963, others practically zero. Diagnosis: ''Ecto, no comments.'' Check: ''Good ecto.''

Individual no. 15. F_I: $-.901$; F_{II}: $-.351$. Diagonsis: ''Ecto, but marked trunk-leg heaviness vs. head-arm.'' Check: ''Yes, I would agree to relatively heavy trunk and legs for this man as contrasted with the head-arm. His head is rather small and his arms are certainly not heavy. His chest is fairly large and his hips and thighs relatively big and rounded. This latter is probably bound up with a rather marked 'g' component in this man.''

CONCLUSIONS

Results. This analysis, of 15 individuals, yields three factors, or modes of differentiation in body shape among them. It is interesting that the number is so small. The first factor reflects the amount of padding between the skin and the bones; the second roughly opposes top-heaviness to bottom-heaviness; and the third, trunk/circum-nasal development to limb development. This kind of result from a factor analysis is not exactly new since Burt, particularly, in trait analyses has repeatedly arrived at a simple bi-polar leptosomic-pachysomic factor, and the limb-vs.-trunk distinction is also familiar. J. M. Tanner and C. W. Harris have each suggested to me that the factors arrived at here might correspond to factors following a general factor in a trait analysis. At the instigation, and with the help, of the latter an attempt was made to see whether this solution could be transformed (see Holzinger and Harman, '41, pp. 290–295) into a solution substantially identical with corresponding parts of a trait-analysis solution (from Howells, '51), without any satisfactory result; the possibility however is not thereby disproved, because of the exceptional nature of the present material. As to the proposition that the result obtained here is simply a repetition of that which would appear in a trait analysis, it seems likely that there are definite differences: (1) the number of ''factors'' is smaller than would be expected from a trait analysis as opposed to a person analysis, and (2) there appear to be legitimate associations — e.g. between trunk size and mid-facial size in factor III — which do not appear in trait analyses, and such associations may be biologically or taxonomically significant. In other words, where a trait analysis explains relationships and distinctions among measured traits through a small number of trait factors, we have here explained relationships in total body shape (always within the limits of 34 measurements used) among a group of persons through a *very* small number of factors. While the results conform to what might be expected from previous trait analyses, the building of a very few definite configurations is what is new herein.

Questions of method. This is where the interest in method lies. If we take one of the individuals, and try to understand his body form through the ordinary anthropometric method, we find ourselves bogged in a mass of 34 trait measurements, each of which would have to be compared to the mean of the trait, and to other measurements, before we could attempt to draw up a description of the man. This confronts us with an impossibly laborious task for use on anything but, perhaps, a fossil skull, and the method also implies nothing at all about the organization of body form. If we turn to somatotyping, we look at a picture, and get a very simple rating, say 7-2-1, which says the man is endomorphic to an extreme, and conveys much information to a student of constitution; this also types the man, putting him with all other 7-2-1's regardless of race, size, etc. It is, at the same time, rather subjective; furthermore, it aims principally at an arbitrarily coded description. The result arrived at in this paper also comes down to a very simple rating, in fact to three scales as in Sheldon's system, but does so metrically; it will give either a simple typing mechanism (grouping people with similar scores, as in somatotyping) or a description of the man, with some implication that the result is significant in itself, because these factors register some kind of natural, biological organization of the differences of individuals.

Now of course this system cannot be put directly into actual use. It is, as presented, under the severest of limitations. It treats, not of body form in general, but of creatures composed of the end points of 34 measurements. Everything about it is based on the interrelationships of 15 specific individuals, and not on those in the population at large. And these 15 individuals are anything but a normal group, being selected with great care for abnormality of form, so that, for example, the first factor may be unduly prominent, accounting as it does here for approximately 57% of the total variance, or 73% of the common factor variance, in the group. Accordingly it cannot be concluded that this set of three factors, or kinds of differentiation, would hold for other groups, or that three is the right number for physique in general. It does not, in fact, seem too likely. It is noticeable that there is in this analysis no factor which may be taken to correspond satisfactorily with a scale of androgyny or gynandromorphy; factor II certainly suggests such a thing, but does not seem really to fill the bill.

Bearing on somatotyping. It remains to be seen, therefore, whether such a method as this can be generalized. Nevertheless, crude as is the present result, it does seem to pose certain useful questions as to Sheldon's components. Since not more than one factor can be associated with endo-ectomorphy, should not the independence of these two components be reexamined? Has not this system been operating as though it were allowed three degrees of freedom when in fact it has only two? (Note that it specifically does not permit the sum of the three ratings to total more than 12 out of a mathematically possible 21.) Possibly, if endo-ectomorphy could be isolated, in Sheldon's system, as a single component, certain other kinds of differentiation might come to light which are now masked by the recognition of endomorphy and ectomorphy as independent components having attributes which are not simply negatives of one another. In a similar way, is it possible that some kind of metrical and statistical approach like the present one is needed to supplement the purely anthroposcopic one, as Tanner ('51) implies? If there are non-Sheldonian kinds of differentiation which, as factors II and III of this study suggest, are consistently and sensibly organized, then should not the concept of dysplasia be reexamined also, to consider what is dysplasia and what merely expresses some normal mode of variation in form?

SUMMARY

Since Sheldon's system of somatotyping, on the basis of three constitutional components, was introduced, no independent method of analyzing total individual physique has been devised which would shed light on the biological validity of Sheldon's system as a whole. In the present study, a person-analysis was performed, by the methods of factor analysis, on 15 individuals (using 34 measurements) who were extremely dominant in endomorphy, mesomorphy or ectomorphy.

Three person-factors, or scaled arrangements of the persons, were found; calculating the composition of these in measurements shows that they do not correspond to Sheldon's three components. The first factor expresses mass, opposing endomorphy directly to ectomorphy; the second opposes top-heaviness to bottom-heaviness (but is probably not equivalent to androgyny); and the third opposes trunk-face development to limb development.

These scales cannot be applied outside of the sample used; however, they raise questions relative to Sheldon's components. Are endomorphy and ectomorphy really independent, or basically only one continuum? Are there other components, not covered by Sheldon's system, discoverable metrically, and perhaps replacing part of what is now classed as dysplasia?

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CASTS OF EAST AFRICAN HOMINOIDS.— Casts of the unique skull of *Proconsul africanus* (the Lower Miocene ape from Rusinga Island, Kenya) as well as of the upper and lower dentitions of the specimen, are now available from the Coryndon Memorial Museum, Nairobi. The price is £5.0.0. including postage

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BRIEF COMMUNICATIONS

A NOTE ON CERTAIN CRANIAL INDICES OF THE STERKFONTEIN SKULL NO. 5

W. E. LE GROS CLARK
Department of Anatomy, University of Oxford

The analysis by Ashton and Zuckerman ('51) of three cranial indices in the Sterkfontein skull No. 5 *(Plesianthropus)* presents certain difficulties of interpretation to which it seems worth while drawing attention. They are difficulties which not uncommonly obtrude themselves when attempts are made at quantitative comparisons between skulls which do not belong to the same species and which differ rather markedly in certain morphological characters. If such comparisons are to lead to valid conclusions, it is of the first importance to ensure that the dimensions studied are morphologically comparable in the strictest sense.

These difficulties do not arise in the comparison of what I have termed ('50a) the "nuchal area height index," and Ashton and Zuckerman were able to confirm my previous conclusion that, in this respect, the Sterkfontein skull not only differs from that of any of the modern large anthropoid apes, but actually comes within the range of human skulls. In regard to the occipital "condylar position index" they likewise confirm my finding that in the fossil skull this may come within the uppermost range of the indices determined in male gorillas if the posterior extension of the sagittal crest in the latter is included in the total skull length. They put forward the argument that "As this index is a measure of the balance of the entire skull relative to the carriage of the body it seems immaterial whether the post-condylar segment is composed entirely of brain-case, or whether it is prolonged by an occipital crest as is usual in the male gorilla." This, however, is not the case. The real measure of balance, it is important to recognize, is the relation of the center of gravity of the whole head to the position of the condyles, and it seems clear that the prolongation backwards of the sagittal and occipital crests into an attenuated flange will not alter to any appreciable extent this center of gravity in the male gorilla. Hence, to include this backward prolongation (which, of course, does not exist in the Sterkfontein skull) in estimating the length of the post-condylar segment must necessarily introduce into the comparison complicating factors the effect of which is very difficult to compute. On the other hand, it is certain, as I have already emphasized ('50a), that in spite of their forward position as compared with apes, the occipital condyles of the Sterkfontein skull "are by no means so far forward relatively to the total skull length as they are in the modern human skull." It is interesting to note, however, that their position relative to the level of the external auditory aperture does correspond with that of *Homo*. There is another point which offers itself for criticism in the comparisons of the condylar index made by Ashton and Zuckerman. In calculating the index, they have measured the post-condylar and pre-condylar segments of the skull length from the lowermost point of the condyle when the skull is orientated in the Frankfurt plane. But since in gorilla skulls (particularly old males) the condyle frequently slopes markedly downwards and forwards, it is evident that in some cases they are comparing indices which are related to the *front* part of the condyle with an index in the Sterkfontein skull which is related to the *middle* part of the condyle. For in the Sterkfontein skull the occipital condyles are (as in human skulls) practically horizontal. Their figures for the relative length of the post-condylar segment of the skull in the gorilla will thus tend to be somewhat exaggerated. It would seem that a more accurate morphological (and functional) comparison is obtained by taking in both cases (as I did in my own studies) the point of maximum convexity of the condyle.

Ashton and Zuckerman also consider the "supra-orbital height index." This index was devised by the present writer in order to test the observation that the *brain-case* in the Sterkfontein skull is set at a higher level relatively to the orbits than it is in the modern large anthropoid apes (and hence appears to approach a characteristically human disposition). In comparing this index, attention was drawn to the fallacy of including in the height measurement the sagittal crest which surmounts the brain-case in male gorilla skulls, for it is evident that this must give quite a spurious value to the brain-case height. Ashton and Zuckerman confirm that the index in the Sterkfontein skull exceeds the range of variation in chimpanzees and orangs, but report that they have records of 15 gorilla skulls in which the index approaches that of the Sterkfontein skull. They indicate, however, that the sagittal crest is included in their estimates of the skull height and, although they do make passing mention of this difficulty in their discussion, they hardly give it due emphasis. Thus, in their summary, they make the simple statement that "the proportion of the cranium above the upper orbital margin (in *Plesianthropus)* is the same as in man and the gorilla." Such a statement, without any qualification, may tend seriously to mislead the reader, and for this reason it seems desirable here to draw attention to a possible source of confusion in appraising the significance of the relative height of the *brain-case* in the Sterkfontein skull.

The comments offered for consideration in this short note are intended primarily to draw attention to a common fallacy which may be involved in attempting to make comparisons of skeletal or other structures by statistical methods — the fallacy of comparing measurements which are not in the strict sense morphologically comparable. For it is clear that, however elaborate the statistical methods employed and however careful the computations, morphological conclusions must be at once invalidated if they are found to be based on dimensions which have no exact morphological equivalence. In the present instance, these difficulties have led to no great divergence of opinion, for the observations made by Ashton and Zuckerman do in fact confirm the conclusions already stated by the present writer ('50a) that "*taken in combination,* (the three indices) appear definitely to place the australopithecine skull outside the limits of variation of the large anthropoid apes and to indicate a rather remarkable approximation to the hominid skull." But all divergence of opinion disappears if due account is taken of methodological differences and of the vitally important principle of morphological equivalence in making statistical comparisons.

Ashton and Zuckerman finally conclude that, if the Australopithecinae did in fact walk upright (and this inference now seems well assured from a consideration of the total morphological pattern presented by the skull structure, the limb bones and, in particular, the three specimens of the pelvis), the head was not balanced as in modern man. This is quite in accord with the conclusion previously reached by the present writer ('50b) that the skull was "balanced more directly on the top of the spinal column than it is in apes (though by no meaus so perfectly as it is in *Homo sapiens*)." It would, indeed, be rather surprising if these primitive hominoids had already achieved an erect posture to the degree of perfection found in man today.

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TELANTHROPUS CAPENSIS AND ZOOLOGICAL NOMENCLATURE

J. T. ROBINSON
Transvaal Museum, Pretoria, South Africa

The brief communication by Osman Hill in this Journal (vol. 9, no. 3) "Note on the nomenclature of certain hominidae" focusses attention on the matter of the scientific terminology of *Telanthropus* and several other described hominids.

According to Osman Hill, "As a generic name *Telanthropus* must fall in view of the earlier usage of the same name by Bonarelli ('09) to include the 'popoli e razze miste' listed as *T. turanicus, arcticus, mongolus, palae-asiaticus* and *(eskimoides) inuiticus (fide* Eickstedt, '37)."

Although quoting from von Eickstedt — about which more will be said below — Osman Hill's relegation of Broom and the writer to the category of "nomenclatural defaulters" in relation to *Telanthropus* is based on the use of the name by Bonarelli as far back as 1909. At this time Bonarelli proposed a classification of the hominids which he has modified and elaborated in various papers since then.

Bonarelli ('46) has given his present ideas on hominid classification in a paper which also gives brief summaries of his previous papers. This was done because Kälin ('45) published a paper on the same subject which ignored Bonarelli's numerous writings. He summarizes his 1909 classification (which I have not seen in the original) and points out that he divides the species *sapiens* of the genus *Homo* into 5 groups which, in the table, he lists as subspecies but gives one a choice of three terms:

". . . . che distingue, nella 'specie,' cinque sottospecie, o varietà principali, o gruppi 'sobolici' (Bonar.) che dir si voglia."

These "gruppi sobolici"— the term he uses most frequently — are as follows:

"— subsp. *Prinanthropus* — Eurafricano-australoidi,
— subsp. *Eitanthropus* — Amerindiani ed Ugro-uralici,
— subsp. *Telanthropus* — Eskimo-eurasici,
— subsp. *Heomixanthropus* — Malaio-polinesiaci,
— subsp. *Hesperomixanthropus* — Popoli europei."

In the body of the paper, giving his present ideas, we find this modified classification:

Gen.: *Homo* Linn. 1759.
Sp. (coll.): *Homo sapiens* Linn. 1759.

1. Gruppo sobolico: *H.s.Eoanthropus* Sm.Woodw. 1913.
2. Gr.sobol.: *H.s.Prinanthropus* Bonar. 1909.
3. Gr.sobol.: *H.s.Eitanthropus* Bonar. 1909.
4. Gr.sobol.: *H.s.Telanthropus* Bonar. 1909.

Each of these "gruppi sobolici" are further subdivided as follows (to quote a single example from the last group):

4. Gr.sobol.: *H.s.Telanthropus* Bonar. 1909.

— *Telanthropus (dordonicus) priscus* (Lapouge) — Chancelade.

In all, the 4 groups are split into 37 such subdivisions, each containing 5 terms.

In a paper published in 1944 Bonarelli also gives essentially the same classification as the above — the "gruppi sobolici" are the same but some of the smaller divisions are not exactly similar. On page 36 he gives an explanation of these groups. He considers *Homo sapiens* to be a "collective" species and the variety of characters to be due to domestication. He quotes the example of domesticated animals and expresses the opinion that the ordinary nomenclatural system cannot cope with the great variety found in such domesticated groups. He therefore proposes the use of a trinomial nomenclature within the usual nomenclature, below the species level, that is the first term (spelled with a capital) in this secondary nomenclature represents *a group within a species*. He then gives a detailed classification, explained at some length, which is quoted in a contracted and slightly modified form in the body of the 1946 paper already mentioned. Bonarelli is not consistent in the name used for the first division below the species level, i.e., the first term in his secondary nomenclature. He most commonly uses "gruppo sobolico" but besides this and the other two terms already mentioned, he also uses "mutazioni." However, he always makes it abundantly clear that whatever name is applied to the groups, they are subdivisions of a species, *sapiens*, the only species of the genus *Homo*.

The above discussion serves to show that although Bonarelli has altered his classification a good deal and uses a cumbersome system which is not in accord with accepted usage in zoological nomenclature, he has consistently used the term *Telanthropus* to designate a group within the species *sapiens*.

From this it is manifest that Bonarelli's use of the term cannot invalidate it as used by Broom and the writer. It is well known — indeed selfevident — that a term applied to a species or a part of a species cannot invalidate, through prior usage, the same term applied to a genus. Taxonomic practice in palaeontology must necessarily differ in some respects from that in neozoology — but this is not the point at issue here. Osman Hill's objection is purely on nomenclatural grounds. Whether his dismissal of the name as invalid can be sustained depends entirely on the correct answer to the question. "Has Bonarelli used the name *Telanthropus* as a generic or subgeneric name?" The answer to this is an unequivocal "No." Therefore, Osman Hill's objection falls to the ground and *Telanthropus capensis* remains valid from a nomenclatural point of view. If at any time this form should be removed from the genus *Telanthropus* and placed in the genus *Homo*, retaining separate specific status, then a new specific name will naturally have to be substituted.

At the time the late Dr. Broom and the writer chose this name we had forgotten the use by Bonarelli of the same name in a different sense. Had we recalled this another name would naturally have been chosen to avoid possible, though unnecessary, confusion such as the present one. Shortly after the name had appeared in print the writer happened to consult Bonarelli's papers again and was immediately aware of the possibility of objection to our use of the term. However, as the name, used generically, was perfectly valid it was not legitimate according to the accepted laws of zoological nomenclature to change it, even to avoid confusion. Nothing could, therefore, be done about altering it.

All of this assumes that Bonarelli's names are valid in the sense used by him. That this is so is doubtful. He has himself said that his terminology does not follow accepted usage. On the face of it, this should automatically invalidate these names and therefore leave them free for future use in any sense. However, this is a point for consideration by those especially interested in the more legalistic aspects of nomenclature.

There is a further point which merits attention. Osman Hill apparently quoted from von Eickstedt's paper of 1937 and did not consult Bonarelli's own writings, which is perfectly in order providing that both von Eickstedt and Osman Hill have quoted correctly. From Osman Hill's quotation, repeated above, one gathers that Bonarelli classified certain peoples as belonging to the species *turanicus, arcticus*, etc., of the genus *T. (Telanthropus)*. This is implicit in the use of only two names, the first spelled with a capital and indicated by the capital letter only. It suggests also that von Eickstedt used the above form in referring to Bonarelli's classification. In fact neither von Eickstedt nor Bonarelli have written anything of the sort. Reference to von Eickstedt's paper shows that he quoted Bonarelli's classification in the form of a table, prefaced by the statement that it is not valid according to the accepted laws of nomenclature. The table shows that the species *Homo (anthropus) sapiens* is divided into 4 groups *Mixanthropus, Telanthropus, Eitanthropus*, and *Prinanthropus*. Each of these is further divided into a number of subdivisions: *turanicus, arcticus*, etc., being such subdivisions of the group *Telanthropus*. This is an entirely different picture to that suggested by the manner in which Osman Hill has reported von Eickstedt. From the table it is perfectly clear that *Telanthropus* is not a generic name nor are the other names quoted by Osman Hill specific names.

Anthropological nomenclature is in a confused state and in view of the power and standing of a name once it gets into print, it surely behooves authors to exercise the most rigid care that any names appearing in their writings are correct. Osman Hill's misrepresentation of von Eickstedt — and through him of Bonarelli — seems to the writer to be inexcusable.

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THE CHÂTELPERRON SKULL

M. F. ASHLEY MONTAGU
Rutgers University

ONE FIGURE

The earliest Upper Palaeolithic culture, formerly called Lower Aurignacian, is now known as the Châtelperronian, after the type site at Châtelperron (Allier) Central France. One of the most enthusiastic collectors and students of this industry, during the latter third of the 19th century, was Dr. Joseph Bailleau, a local French physician. Some years ago the Wellcome Historical Medical Museum of London purchased Bailleau's collection from his executors. In the collection was an undescribed fossil human calvarium.

The artifacts provided the materials for a definitive monograph on the Châtelperronian industry by Mr. A. D. Lacaille ('47) in which, as an appendix, there was contributed a study by Dr. A. J. E. Cave ('47). In the summer of 1948 I had an opportunity of examining the skull at the Wellcome Historical Medical Museum. I later published a brief note on the skull (Montagu, '49), and it is described and illustrated with Cave's three original figures in a later work (Montagu, '51).

Through the courtesy of the Director of the Wellcome Historical Medical Museum, Dr. E. Ashworth Underwood, I am able to reproduce a photograph of the Châtelperron skull, which was originally printed in a recent publication of the Museum (Underwood, '51).

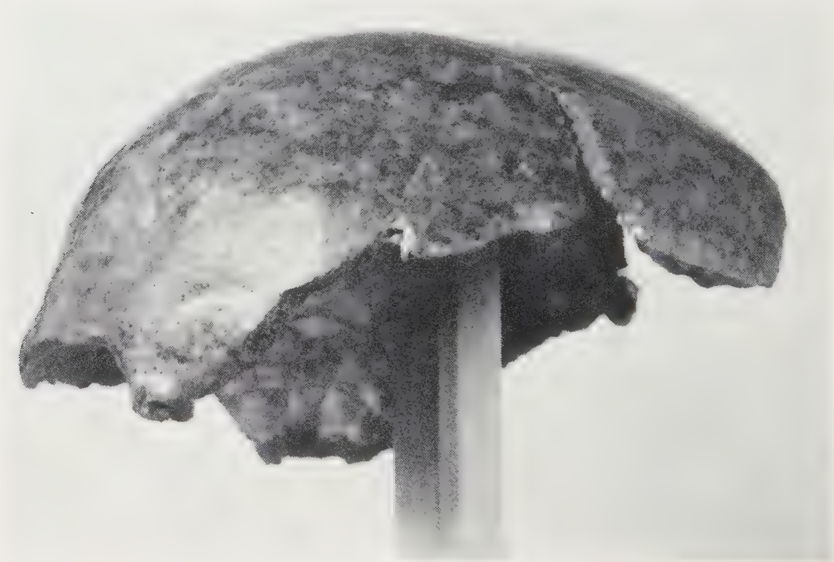

Fig. 1 The Châtelperron skull. From figure 3, Underwood, '51. Courtesy E. Ashworth Underwood and The Wellcome Historical Medical Museum.

There is not known to exist any account of the discovery of this skull; the presumption is that it was discovered by Dr. Bailleau in the multiple cave of La Grotte des Fées, in the commune of Châtelperron, from which most of the original Châtelperronian artifacts came.

The calvarium is extraordinarily heavy, and corresponds in degree of mineralization and hue with other fossil bones recovered from the cave of La Grotte des Fées. It consists of the entire right and the greater part of the left parietal, the whole of the right and the greater part of the left frontal. The right, and only remaining, supraorbital margin is sharp and gracile, there is no evidence of a supraorbital torus, and the form of the naso-frontal process indicates that there was a prominent nasal skeleton. The bones are extremely thick: 10 mm at the thickest part of the medio-frontal region, and 11 mm at the thickest part of the parietals. Maximum length is 186 mm and maximum breadth 158 mm the cranial index being 85.5, that is, hyperbrachycranic. Cranial capacity is estimated at about 1,425 cm^3.

Somewhat broader and flatter in the vault, and generally thicker, the Châtelperron skull in its contours is not unlike the later Aurignacian, Solutrean, Předmost, and Obercassel skulls.

It may be helpful to point out that culturally Châtelperronian precedes the industries (Aurignacian) associated with Crô-Magnon man.

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ON THE FREQUENCY OF SHOVEL-SHAPED INCISORS IN THE FINNS

KALEVI KOSKI AND EEVA HAUTALA

Institute of Dentistry, University of Helsinki, Finland

THREE FIGURES

The occurrence of the shovel-shaped incisor has been the subject of a great many studies, as can be seen from the careful reviews in Pedersen's ('49) and Dahlberg's ('51) publications. The findings so far seem to indicate that this peculiar morphological feature might be a characteristic of Mongoloid people (cf. Dahlberg's composite tables); slightly differing statements to this effect have been made by several authors.

Our information regarding the frequency of the shovel-shaped incisor in racial groups other than Mongoloids, however, is very scanty. As to the Whites, the only figures available seem to be those published by Hrdlička ('20) for American Whites, indicating a low frequency.

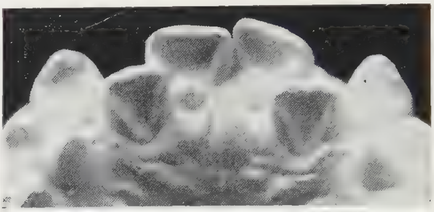

Fig. 1 Pronounced shovel-shape of all incisors.

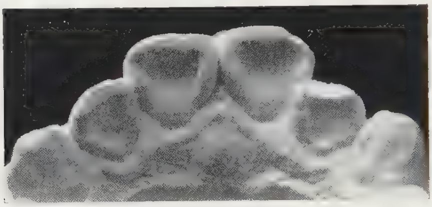

Fig. 2 Moderate shovel-shape of all incisors.

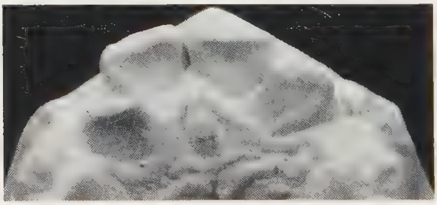

Fig. 3 Faint shovel-shape of all incisors.

While examining the patients in the Orthodontic Department of the Institute of Dentistry, University of Helsinki, our attention was drawn to the apparently high incidence of shovel-shaped incisor in that group. The impression gained by clinical inspection was surprising enough to arouse our curiosity and to initiate further investigation on the subject. It is our intention to give in this short note the results of a sample test to indicate the frequency of the shovel-shaped incisor in the Finnish population.

The total number of individuals included in the survey was 212, of whom 92 were listed as orthodontic patients, the rest being dental students. After an attempt to evaluate the morphology of the incisors in the mouth, which we soon found to be extremely difficult, it was decided to use plaster casts. We already had casts of the patients, made by using a hydro-colloidal impression material. For taking impressions of the students' teeth, a special dental wax was used, which made the procedure easier and quicker, but gave reproductions accurate enough for our purposes. Care was taken to exclude all teeth in which attrition or other factors had made a reliable determination of the morphology impossible.

Since this survey was of a preliminary nature, we thought the visual method should suffice in evaluating the occurrence of the shovel-shape. In order to reach a degree of objectivity, we used Pedersen's photographs (Pls. 29, 30) as a basis on which to compare our casts. The specimens in figures 1 to 3 may serve to illustrate our conceptions of the different degrees of shovel-shape which, according to Pedersen, is classified as pronounced, moderate, or faint. By working together, counterchecking each other's evaluations, we have hoped to increase the objectivity of our findings.

TABLE 1

The occurrence of the shovel-shaped incisors in the Finns

	MEDIAN	LATERAL
Pronounced	16 3.8	12 2.9
Moderate	46 10.9	68 16.7
Faint	323 76.4	299 73.3
Absent	38 9.0	29 7.1
	423 100.1	408 100.0

While looking through the material it was found that even the right and left antimere sometimes show different degrees of shovel-shape, not to speak of median and lateral incisors, which often differ in the intensity of this phenomenon. Thus it was considered suitable to present the findings according to individual teeth.

The figures for the total material are presented in table 1, both in actual numbers and in percentages. The total frequency of any degree of shovel-shaped incisor for the median incisor is 91.0 ± 1.4% and for the lateral incisor 92.9 ± 1.3%. As Dahlberg has suggested, a safer way to compare the findings of different authors who have used visual inspection would be to count the pronounced and moderate forms only. If this is done we get: I_1, 14.7 ± 1.7%; I_2, 19.6 ± 2.0%.

TABLE 2

Comparative data (from Hrdlička, '20) indicating the position of the Finns among racial groups (in percentages)

MEDIAN INCISOR

		Pronounced	Moderate	Pronounced + moderate	Faint	Absent
American White	M	1.4	7.6	9.0	24.5	66.5
	F	2.6	5.2	7.8	21.8	70.4
American Negro	M	4.9	7.6	12.5	33.0	54.5
	F	3.6	8.0	11.6	32.6	56.0
Finns	M	3.6	12.6	16.2	76.6	7.2
	F	3.8	10.3	14.1	76.3	9.6
Chinese	M	66.2	23.4	89.6	1.8	7.8
	F	82.7	12.5	95.2	1.0	3.8

LATERAL INCISOR

		Pronounced	Moderate	Pronounced + moderate	Faint	Absent
American White	M	1.4	8.8	10.2	36.4	50.0
	F	1.0	7.4	8.4	29.9	59.6
American Negro	M	4.5	12.8	17.3	38.0	42.1
	F	3.8	11.1	14.9	35.1	47.5
Finns	M	3.7	20.2	23.9	68.8	7.3
	F	2.7	15.4	18.1	74.9	7.0
Chinese	M	56.9	24.0	80.9	1.5	9.5
	F	68.8	13.5	82.3	1.0	3.4

The claim that the lateral incisor is more affected by shovelling is to some degree supported by our findings also, although the difference here is not significant. There is no significant sex difference, either (as can be seen from table 2). At first the impression gained by clinical inspection seemed to point towards a difference between the orthodontic patients, most of which had crowding of teeth, and the students (considered to represent a cross-section of the population) in relation to the intensity of shovel-shape, but the final figures disproved this. Thus the assumption of a correlation between the incidence of shovel-shape and crowding of teeth, held by us for some time during the collection of material, was found to be invalid.

To illustrate the position of the Finns among other racial groups regarding the frequency of the shovel-shaped incisor, our findings are presented together with Hrdlička's figures (table 2) for American Whites, American Negroes and Chinese, who seem to be the closest "relatives" of the Finns, the Chinese having the lowest frequency among Mongoloid people. If one looks at the total frequency of the shovel-shape, the Finns come very close to the Chinese, far ahead of the Whites and Negroes. When Dahlberg's suggestion is followed — in the column "pronounced + moderate"— it can be seen that the Finns are still close to the non-Mongoloid groups. However, the difference between Hrdlička's Whites and the Finns is significant in every instance. It must be pointed out that our figures may not be fully comparable with Hrdlička's, since we do not know his standards for the intensity of shovelling, and his figures are based on individuals, not on teeth.

In regard to the implications of our findings, there is not much to be said. As to the long disputed problem of how much of a Mongoloid element exists in the Finnish population, it is interesting to note that recent serological discussions (Ashman, '50) incline to minimize the influence of that element in the Finns, although it is stated that a minor admixture may have occurred. If the shovel-shaped incisor really is a Mongoloid characteristic, our findings may be considered to support this opinion. However, our knowledge of the nature of the matter under discussion appears, at the moment, insufficient to permit any definite conclusions to be made. We do not know the mode of origin and transmission of the shovel-shape, nor do we know of its incidence in more than a few racial groups. Further studies on the subject with special inclusion of various racial and sub-racial groups, and the use of more accurate and objective methods of investigation, as advocated by Dahlberg, are strongly indicated.

ADDENDUM

After this paper had been prepared and sent for publication, the authors received a personal communication from Dr. Gabriel Lasker, concerning his mention about the frequency of shovel-shaped incisors among American Whites, in his recent paper "Genetic analysis of racial traits of the teeth" (Cold Spring Harbor Symposia, Vol. XV, 1950). He states there that 14% of his material showed moderate or greater degree of shovel-shape, while 50% of the population possessed at least some tendency towards the trait. Although his standards and classification, according to the communication, are somewhat different from those used by us, his figures seem to support the ideas expressed in our paper regarding the need for further studies and more objective, uniform methods.

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''GROWTH OF GUAMANIAN CHILDREN''— SOME METHODOLOGICAL QUESTIONS

JAMES A. GAVAN

Yerkes Laboratories of Primate Biology, Inc., Orange Park, Florida

ONE FIGURE

In his recent study on ''Growth of Guamanian Children,'' Greulich ('51) found that the Guamanians were smaller in weight, stature, and sitting height than the Brush children of the same average age. He then turned to the weight-height index which he describes as a somewhat arbitrary but useful device for comparing ''. . . the weight per unit height of individuals'' (p. 64). Thompson ('48) had already shown that the cube root of weight, rather than weight itself, is the volumetric measure which should be compared to a linear dimension. Huxley ('32) and Sholl ('50) have shown the utility of the concept of regression in growth. Although the combination of these methods, cube root of weight compared with stature by means of regression, may be as arbitrary as Greulich's method, it seemed worth while to determine their usefulness.

The data for weight and stature given by Greulich were used. The cube root of average weight was determined and plotted against average stature as shown in figure 1. This method of handling data is quite different from the index method. When the regression is linear, so that the equation $Y = a + b\ X$ (in the present case let Y = cube root of weight and X = stature) fits the data, then the index from the same data will not be linear. This is easily seen by dividing the equation by X and multiplying by 100:

$$\frac{100\ Y}{X} = \frac{100\ a}{X} + 100\ b.$$

The left side is now an index while the right indicates how the index changes with increase in size, or age if the index is so plotted. As "*a*" and "*b*" are constants the value of $100\ a/X$ will be large when X is small and will decrease as X increases. Therefore, when two measures show a constant relation via a regression equation they will show a changing relation via an index.

Returning to figure 1, it can be seen that the 4 groups (boys and girls, Brush and Guam) follow the same curve. The two female groups eventually depart from the curve, Guam at about 151 cm stature and Brush at about 165 cm stature. This indicates that linear growth, stature, is completed sooner than weight growth.

After omitting the last three measures of weight and stature for the two female groups (the measures for which stature has ceased but weight has continued to increase), a straight line was fitted to the data using the method of least squares (Fisher, '46). This equation, given in figure 1, has predictive value. Within the range of the data used in its derivation, it is possible to predict the most likely value of the cube root of weight for any stature; the one equation completely summarizes all 4 sets of data. On the other hand, because the index changes with increase in size such prediction from an index is impossible.

In order to justify the use of a linear equation, the standard deviation of the cube root of weight ($s_Y = .3969$)[1] and the standard error of estimate for the regression ($s_{Y \cdot X} = .0184$) were calculated. (Read $s_{Y \cdot X}$ as the standard deviation of Y which is independent of X.) It can be seen that only about 5% of the original variability in the cube root of weight is not associated with the variability in stature ($.0184 \times 100/.3969 = 4.63$). Considering the diverse origin of the data, this amount is negligible.

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[1] The standard deviation of the present available data is represented by s while σ is reserved for the standard deviation of the universe of data from which the present sample was drawn.

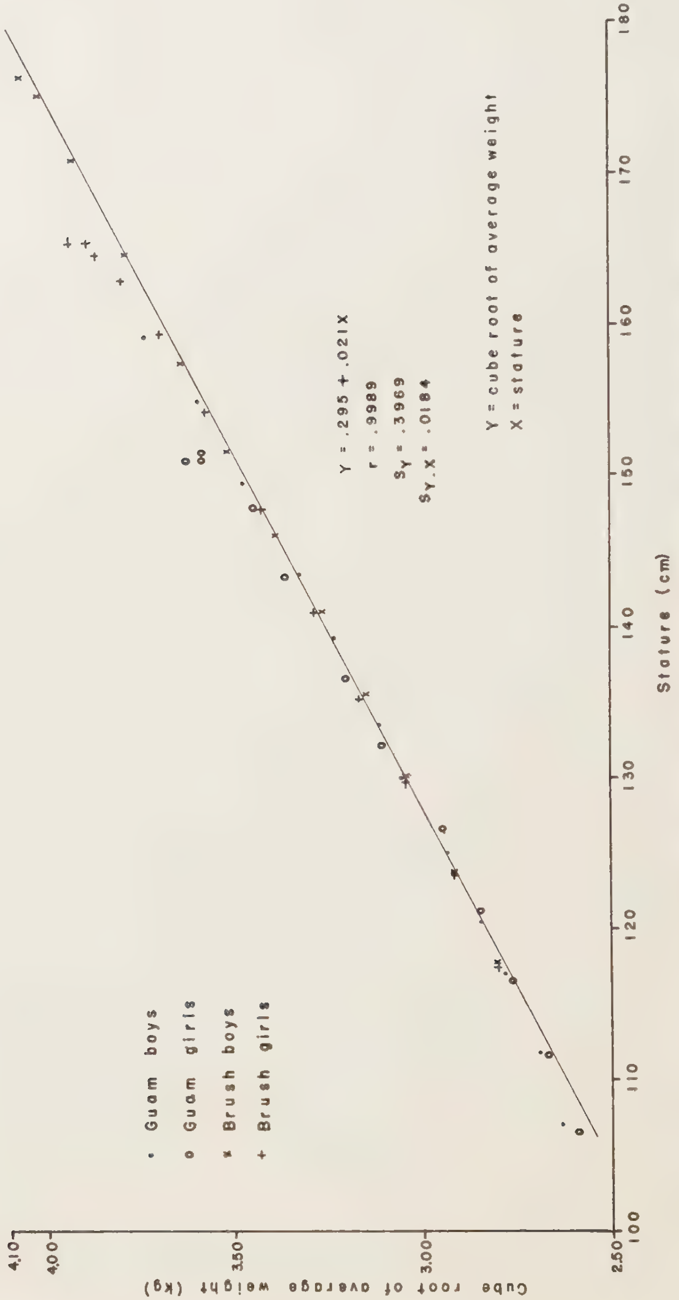

Fig. 1 Cube root of average weight plotted against average stature for the Brush and Guam boys and girls.

Much the same picture is given by the correlation coefficient ($r = .9989$). The two measures have practically all of their variability in common.

The position of the Brush children in the curve is not the same as that of the Guamanian children. The latter enter the curve at about 107 cm stature while the Brush children enter about 11 cm later. That is, even though the weight-stature relation is the same in all groups the Brush children are absolutely larger than the Guamanian. Assuming that part of this absolute size difference is due to the deleterious environment under which the Guam children lived, it must have affected weight and stature in a dimensionally equivalent manner. That is, the Guam children are no more underweight for their stature than the Brush children and the Guam boys are no more underweight for their stature than the Guam girls. Analysis of the height-weight index led Greulich to conclude that the Guam sex difference was greater than the Brush and, therefore, the Guamanian boys were more underweight than the girls.

Conclusions so different from those arrived at by Greulich raise some questions concerning methodology.

1. Should weight itself or the cube root of weight be compared to a linear dimension? The choice would appear to be a matter of convenience. Either one will show a constant relation, but if weight is used the relation will be curvilinear rather than linear. As the latter is easier to handle it would appear preferable.

2. Should comparisons be made via an index or a regression line? When the purpose in comparing two groups is to determine how the dependent variable (here weight) changes in relation to the independent variable (here stature), regression is the more useful method. Greulich's figure 5, weight-height index by age, appears to show only what he had already demonstrated in figures 2 and 3, namely, that at the same age Brush children are larger than Guamanian children.

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* * *

Corrigendum.— In "The geological age of Pithecanthropus, Meganthropus and Pithecanthropus," by Dirk Albert Hooijer, in the September, 1951, issue of the Journal (vol. 9, no. 3), there occurs on p. 272 the sentence:

"Dr. Teilhard de Chardin has always regarded it as more convenient, for China, to take Villafranchian as Upper Pleistocene. . . ." The last two words should read "Upper Pliocene."

The "EEL" Reflectance Spectrophotometer.— One of the most pressing and long-standing requirements in physical anthropology is for an objective method of skin-colour measurement. That reflectance spectrophotometry of the skin has not come into general use is no doubt due to the fact that the elaborate Hardy instrument of the Massachusetts Institute of Technology was (and probably still is) quite unique and that no simpler apparatus for general laboratory or field work has been forthcoming, although a relatively simple photo-electric reflectance colorimeter for use in the paint industry was described by Bolton and Williams as long ago as 1937. The portable trichromatic colorimeter suggested still earlier by Williams ('33) would not give a performance comparable to that of the Hardy type of instrument or to that of Bolton and Williams.

A photo-electric spectrophotometer — the "EEL" Reflectance Spectrophotometer made by Evans Electroselenium, Ltd., Harlow, Essex — has recently been put on the market, intended for industry, which gives spectral reflectance values of the human skin comparable to those of the Hardy instrument and makes possible also the specification of the colorimetric properties of the skin. In view of its portability and simplicity it would appear to be suitable for anthropological field work. The instrument appears in most respects to be similar to that of Bolton and Williams ('37).— J. S. Weiner. A spectrophotometer for measurement of skin colour. Man, vol. 51, no. 253, Nov., 1951, pp. 152–153.

REVIEWS

THE MEANING OF EVOLUTION: A STUDY OF THE HISTORY OF LIFE AND ITS SIGNIFICANCE FOR MAN. By George Gaylord Simpson. xv + 364 pp., 38 figures. Yale University Press, New Haven Connecticut. 1949. $3.75.

This volume, by the distinguished author of "Tempo and Mode in Evolution," is the most important general synthesis of the meaning of evolution to appear in modern times. Professor Simpson has been primarily responsible for the successful synthesis of paleontology and genetics; any contribution from his pen is worthy of the serious attention of all anthropologists and is required reading for physical anthropologists. Since this volume represents in a considerably expanded form the 25th series of Terry Lectures, delivered at Yale University in November, 1948, that sponsoring institution, together with the author, deserves our professional gratitude. The scope of the volume is best summarized in Professor Simpson's own words:

> "The general plan of the enquiry has three phases which correspond approximately with attempts to answer three questions: What has happened in the course of evolution of life? How has this been brought about? What meaning has this in terms of the nature of man, his values and ethical standards, and his possible destiny?"

These questions have neither been posed nor answered lightly.

Part I, "The Course of Evolution," compactly summarizes the substantive materials of evolution. In the author's hands, these data are presented in new perspective. Following Knopf, the geological time scale is extended so that the Cenozoic is given a duration of 75 millions of years. Consequently, more time is now available for primate evolution. The latest views concerning the origin of life and the evolution of the original protogene through the long developmental period to the amoeba stage are summarized. On the vertebrate level, he notes that among the 4 classes equipped with fins for swimming in water (conventionally called fishes), the inter-group distinctions in the fundamental anatomy are more marked than between the reptiles and mammals among land animals. Vertebrate evolution in the Mesozoic is illustrated with conventional data, but interpretations result in inferences which may be unfamiliar to non-paleontologists. For example, speaking of the reptile-like brain of all early and few late mammals:

> ''A typical mammalian level of intelligence, then, is not an original or early characteristic of the Mammalia, but has developed independently and rather differently in each of the different groups of mammals that show it. We do not know how many such independent increases of intelligence there have been, but apparently a large number.''

Professor Simpson finds that the concept of parallel evolution holds no terrors.

Physical anthropologists will be interested in the author's treatment of primate evolution. Amply aware of the classificatory difficulties in this field, they will not resent his candid estimate:

> ''Primate classification has been the diversion of so many students unfamiliar with the classification of other animals that it is, frankly, a mess. It involves matters of opinion on human origins and, humans being what they are, such opinions are needlessly varied and not always distinguished by competence or logic.''

Four general levels and type of structure are distinguished among the primates: (1) the prosimians, which include the living lemurs and the tarsiers; (2) the ceboids; (3) the cercopithecoids; and (4) the hominoids, comprised of the 4 great apes and man. Paleontological evidence for the evolution of the primates is better than anthropologists have presumed.

> ''The hominoid record is surprisingly good, with fewer complete breaks than in the cases of the other groups. Still there is a gap in the African Pliocene record, and earlier and later fossils from that continent show beyond much doubt that it was a, perhaps *the,* major center for this group in the Pliocene, also. The known Miocene and Pliocene hominoids are not likely to represent as many as half those actually existing at those times, even though we already know a greater variety of hominoids in those epochs than exists today.''

These reassurances will provide encouragement to those who suspect that insufficient data are now in hand for a complete and neat linear reconstruction of hominoid descent.

Professor Simpson presents no detailed reconstruction of primate evolution, but he notes from the fossil record two important periods of radiation. From an early Tertiary prosimian radiation, the three other primate lines arose. Thus the ceboids, cerecopithecoids, and hominoids originated at about the same time level, and do not represent successive steps in primate development. A second period of radiation affected all 4 groups in the Miocene. Hominoid radiation is interpreted as reaching an early Miocene climax and since that time there has been a weeding out of its lines, many of which became extinct. The relatively few surviving lines show an increasing specialization and divergence. The various australopithecines of South Africa may be evaluated as being the extinct products of a late radiation of one hominoid line, which probably occurred in the Pliocene. Modern man is the surviving product of this phase. Or alternatively they may merely be another branch from the original Miocene radiation. It is considered improbable that they will prove to be actually human ancestors. Professor Simpson considers that in the present state of our knowledge these hypotheses turn on disputed details which tend to confuse, rather than clarify, the broader picture of primate evolution. Part I is concluded with an important discussion of rates of evolution. Its compactness defies summary; it must be read for full appreciation.

Part II, ''An Interpretation of Evolution,'' is concerned with the question of what forces have been acting throughout the history of life. Materialist, vitalist, and finalist views are summarized. In an acute discussion of orthogenesis, the major orienting factor in the process of evolution is identified as adaptation. But it is admitted that the opportunism of evolution requires recognition that the history of life is an odd blend of the directed and the random, the systematic and the unsystematic. Mutation provides the random aspect which, combined with the directive force of selection, gives the best possible explanation of the major forces operative. This section is concluded with a critical evaluation of the concepts of racial life and death, and of evolutionary progress.

Part III, ''Evolution, Humanity, and Ethics,'' is devoted to the evaluation of man's place in nature; the search for an ethic; and the future of man and of life. Professor Simpson is revealed as the rare natural scientist who also observes the broader social obligations inherent in his field.

In October, 1951, an abridged version was published as a Mentor Book by the New American Library of World Literature, Inc., N.Y.C. The 9 chapters of Part I of the original book have been radically abridged and compressed into a single chapter in the Mentor version. The remainder of the book is but little changed, and it makes an excellent, inexpensive supplementary text for courses in physical anthropology.

Joseph B. Birdsell
University of California at Los Angeles

DER GEISTIGE AUFSTIEG DER MENSCHHEIT VOM URSPRUNG BIS ZUR GEGENWART. By Hans Weinert. Second revised edition. viii + 303 pp., 132 figures. Ferdinand Enke Verlag, Stuttgart, 1951. DM 19.

Dr. Weinert's book is part of a trilogy, the two preceding studies having dealt with the Genesis of Mankind and the Origin of Human Races. In his preface the author informs the reader that this last volume (The Mental Ascent of Mankind from Origin to Present) does not represent a psychological, archaeological, or ethnological study, or a treatise in cultural history. This reviewer is at a loss to understand what else it might be called. It seems to be what in this country would be classified as an archaeological textbook.

The book starts with a discussion of the mental achievements of apes. Some of the material presented is taken from Yerkes (persistently misspelled Jerkes), Koehler and others. Dr. Weinert's own observations contribute some delightful stories. The chapter *Menschwerdung als geistige Tat* (a title which is rather difficult to render into English: "Becoming a Human — an Act of the Mind") stresses the importance of the use of fire in the evolution of man. Weinert plays with the idea that change in food habits caused by the use of fire might have caused changes in metabolism and resulted in mutations (p. 63, 65). The use of fire greatly increased the amount of work which man had to do, since it necessitated constant care (p. 59). Forced to work so much more and so much harder, man began to grow conscious of his activity and to grasp its meaning (p. 60). In the same breath, however, Weinert tells us that fire permitted man to delight in leisure; snugly warmed and enjoying his spare time, man could form ideas which formerly the rush of his way of living had prevented, and so made the first artifact (p. 66).

The following chapters attempt to describe the civilizations and the physical appearances of Propithecanthropus and Pithecanthropus, of Neanderthal Man, Homo sapiens diluvialis, Homo sapiens alluvialis, and finally of Homo sapiens recens of the Metal Age, and to infer the mental stages reached by each.

The author's thesis is that the mental stage always equals the physical form (p. IV; also III, 3, 66, 71). However, he admits that the differences in human brains which should be expected according to his theory have not yet been demonstrated (p. 5). Furthermore, Dr. Weinert calls the neolithic era a turning point in man's mental rise without apparent physical change (p. 290). He even says that man, although representing the highest stage of mental evolution, is not the mammal with the highest physical development (p. 54).

The volume of 1951 represents a revised edition of a book originally published in 1940. To a certain degree it is up-to-date. For example, the Australopithecinae are discussed. On the other hand, the author seems to be unaware of the doubts concerning racial classification which some anthropologists in this country have expressed. There appear terms like Nordic, Alpine, Dinaric, etc., as well as European race (p. 280) and Europid race (p. 205; 240; 275). The European is superior: no other races have devised or worked out anything really new (p. 275). Only some members of the Nordic race are capable of the highest mental achievements (pp. 280–282). The present mental apex is linked inseparably with the author's own race, and to say so does not mean to boast (p. 293). No Negro nor any person of any other colored race is able to reach the mental summit of the European (p. 292). Only one race supplies the individuals who create progress (p. 286). The mental differences between the highest and the lowest human is greater than the difference between the lowest human and the highest ape (p. 4; 20). It is even doubtful whether all humans living today belong to Homo sapiens (p. 48). The diversity of mental and cultural achievements prove that the use of just one word, "man," covers too many varied elements, and is, therefore, "totally unjustified" (p. 219).

It will be recalled that German anthropologists at their first postwar meeting in 1948 protested against the charge that German anthropology was to blame for, or had any part in, the exaggerations *(Überspitzungen)* of Nazi racism. Only complete ignorance, it was said, could make such a charge, and the very opposite of it was true.

Dr. Weinert's racist ideas, of course, are by no means new. He himself has presented them repeatedly in previous books. The dogma of superiority was taught in a similar way by Eugen Fischer and others. That it is told anew in 1951 poses, it seems to me, a problem. Could it be that such views are not considered *Überspitzungen* by the authors of the declaration quoted, but proved facts and simply the truth? I should like to know since I, for one, do not like to be accused of "complete ignorance."

PAUL LESER
The Hartford Seminary Foundation

GRUNDZÜGE DER VÖLKERBIOLOGIE. By Ilse Schwidetzky. xi + 312 pp., 55 figures. Ferdinand Enke Verlag, Stuttgart, 1950. DM 17.

The growing interest in this country in human ecology assures Miss Schwidetzky's book of many an attentive reader. It is well written and well organized. Documentation is ample. Dr. Schwidetzky's knowledge of the literature, even foreign literature, is commendable, nay, admirable, if one considers the war and postwar difficulties with which she had to cope. There are, of course, omissions. The choice of different material might have given some chapters or even the book as a whole a different slant.

Following Scheidt and Thurnwald, Dr. Schwidetzky makes a point of distinguishing between selection and *Siebung*, "screening". Selection eliminates the unfit. Screening redistributes variants in such a way that those unfit for a certain environment are put into one for which they are fit. Examples: "Local screening" puts individuals into the geographical area for which they are suited, whether it be forest or steppe, urban or rural environment. "Social screening" puts individuals into social groups for which their abilities are presuppositions: dancers are a group screened for nimbleness, scholars one screened for intelligence. Screening, however, frequently is linked with, or leads to, selection.

The book is divided into three main parts. The first part discusses the influence of *migrations* on human groups and on the human organism. Two familiar rules are mentioned which determine the distribution of man on earth: (1) the progressive races occupy the more desirable areas and push the primitive races back into the less desirable ones; (2) the primitive races are living in marginal areas, farthest away from the large central land mass of Asia. Political, social, economic pressures force man to migrate, and so does the wandering impulse, which is a hereditary trait. Thus migration constitutes a screening process which causes important changes in the character of the group that stays behind as well as of that which leaves.

Europids are considered far more mobile than any other "race". (The Arabs and the Polynesians are considered Europid, but not the population of India.) Dr. Schwidetzky distinguishes three centers of unrest: the Nordic center (Southern Scandinavia and the north of continental Europe); the Central Asiatic center (Mongolia and Eastern Turkestan); and the center in Asia Minor (Arabian peninsula and vicinity). All three areas are of little economic productivity. A change in climate or an increase in population therefore causes an immediate pressure which easily leads to migration.

Migration means screening according to sex and age. More men than women migrate. More persons between 15 and 30 migrate than of other age groups. Whether a specific morphological type is more frequent among those migrating seems doubtful.

Migration also results in selection since the difficulties caused by every change of environment act as selective factors. Women, and especially children, adapt themselves physically less readily to environmental changes than men, but mentally *(seelisch)* more readily. Schyzothymes (who seem to be more inclined to migrate) are less adaptable to change than eurysomes. Mongolids, on the whole, are better adaptable to different climates than Europids or Negrids, but the Japanese are not.

The second part of the book discusses the relationship of *social environment* to physical type. In the fight for social status as well as in the battle for space the progressive type is said to be always victorious over the primitive type. Social stratification is maintained by prohibiting intermarriage and by excluding the lower group from certain jobs. Assimilation depends on the ability of one of the groups involved to admit and absorb strangers and of the other group involved to adapt itself. It is not always the larger group that assimilates the smaller one. Assimilation causes changes in the structure of the peoples involved. The more different the two original groups were the wider will be the psychological and morphological range of the new population and the greater its possibilities.

Social screening is called the main problem. The general level of intelligence runs parallel to social status. People going into different vocations differ not only as to their talents but also as to their constitutional types. However, vested interests of many kinds prevent limitless screening that would regularly channel the proper person to the proper place. A fully flexible society never existed nor did a fully stagnant caste system.

Social types differing morphologically *(morphologische Sozialtypen)* are something that can be seen and grasped *(sichtbar und greifbar)*. Streetcar conductors in Vienna are impressively more Easteuropid, pavement workers more Nordic than the average. The higher classes always are taller than the lower classes. This rule holds true of "all nations and continents" and is called one of the few truly established facts of social typology. However, exceptions to the rule are admitted. Shoemakers are always the shortest group within a population in spite of the fact that they do not belong to the lowest class.

Ammon's law claimed that cities attract more persons of Nordic race than others. Dr. Schwidetzky admits that there is no basis for Ammon's assumptions.

The third part of the book is devoted to problems of *reproduction.* Women marry more frequently than men into a higher social group. Victors marry the women of the vanquished. Mates are not chosen at random but from a relatively small group, usually from the immediate vicinity. Inbreeding is frequent, even where not easily recognized as such. As examples, Dr. Schwidetzky quotes two cases where in the 4th generation of ancestors there were only 49% and 36% of the theoretically expected number.

The investigation of population problems culminates in a discussion of Malthusian ideas. Then the concept of selection and the importance of selection for the formation of races is examined. There follows a review of the selective effect of infant mortality, epidemics and famines, war, and finally, of civilization.

Notwithstanding its heading, chapter 4 of part 3 deals with differential fertility among so-called civilized peoples. The last chapter of the book devotes itself to population dynamics from the decline of primitive peoples to the future of the Europids.

It is, of course, quite impossible, in a review of a few hundred words to do justice to a book of over 150,000, and especially to one that so abounds in ideas and material. Dr. Schwidetzky presents many theories of her own and quotes an overwhelming number of hypotheses of others; quite a few of both should give rise to doubts. As can be gathered from this review, Dr. Schwidetzky, as a pupil of Baron von Eickstedt's, generally follows his terminology and his classification of races. Also, some of the old bias of the Ploetz brand of "*Sozialanthropologie*" survives. So do some of the dubious old methods. "Progressive" traits of the Europids are named, but not their primitive ones, nor the "progressive" ones of the Negrids. Small samples are quoted. Even if the ratio is as high as 7 former prostitutes out of a total number of 29 white wives of a certain group of Negroes, the numbers seem too small for any conclusion. Some groups are not comparable at all. The populations of China and Germany cannot be compared in their entities in regard to migratory habits. Only those groups of Chinese and Germans would be comparable who live at the same distance from seaports, whose economic conditions were similar, who were subject to similar discriminatory immigration laws, etc. Strange generalizations creep in. Wealth is claimed to rank "everywhere" above poverty in social esteem, while a look at India or at the publican in the New Testament could have prevented such a baffling statement.

However, in general Dr. Schwidetzky tends to be rather careful and succeeds more often than not in recognizing and presenting the diverse views of the problems she deals with. Her book, and this may be considered its greatest merit, quite consciously poses more questions than it tries to answer. A comparison of Miss Schwidetzky's work with Ploetz and similar studies shows what tremendous advance it signifies.

Paul Leser
The Hartford Seminary Foundation

THE MASTOID PROCESS IN THE CHIMPANZEE AND GORILLA

E. H. ASHTON AND S. ZUCKERMAN
Department of Anatomy, University of Birmingham,
Birmingham, England

THIRTEEN FIGURES

INTRODUCTION

The mastoid process is defined as a downward mamillary projection of the mastoid portion of the temporal bone behind the external acoustic meatus. To the medial side of its base is a deep groove, the digastric fossa (mastoid notch), for the origin of the posterior belly of the digastric muscle, and medial to that is a less distinct groove for the occipital artery. The free medial surface of the process is usually flatter than the lateral surface, which gives insertion to the *sternomastoid, splenius capitis* and *longissimus capitis muscles.* The process is variable both in size and shape, and is described as usually being larger in the male than in the female human skull. According to Martin ('28) it also varies in prominence between different "races" of man. Schultz ('17), in a study of 202 skulls of both sexes and several racial types, found that the distance from the lower tip of the process to the porion, the highest point of the external auditory meatus ("both projected on a sagittal plane and on a line perpendicular to the horizon, glabella-basion, representing the skull base") varied over the wide range of from 10 mm to 27 mm.

It is often stated that a mastoid process as defined in this way is a human characteristic, and that a freely projecting process is absent in the skulls of the great apes (especially the chimpanzee and orang), where the mastoid bone is believed to have a roughened but uniformly rounded external surface.

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Thus Weidenreich ('43) writes as follows: "in modern man the mastoid portion [of the temporal bone] consists of two parts, a larger and more protuberant anterior one, represented by the mastoid process, and a smaller, flatter, posterior one which has a squama-like character similar to the squamae temporalis and occipitalis. Concerning their topographical relation to the squamous and petrous portions both parts lie to the rear of the latter but the protuberant one generally below the squamous portion if the supramastoid crest is taken as level. In anthropoids there is only one undivided mastoid portion situated behind the squamous portion, in orang-utang and gorilla even high above the level of the supramastoid crest. There still is another difference between man and anthropoids. In the former the protuberant part can be subdivided into two portions, an upper and a lower one. The lower one, the mammillary process, projects freely downward while the upper one merely represents its base and extends upward to the supramastoid crest. In anthropoids, a free process is lacking; instead the entire portion bulges laterally and carries on its surface a broad-rooted ridge which continues posteriorly into the nuchal crest." Schultz ('50) has recently pointed out that this view is incorrect, and that in the gorilla the mastoid process may "compare favorably with the size of this structure in man and this even among females." It is always larger, according to Schultz, in the gorilla than in the orang or chimpanzee in which it is at best "poorly developed" — although there are exceptional skulls with a very prominent mastoid process. Schultz studied 25 male and 10 female gorilla skulls, but does not indicate how many specimens of chimpanzee and orang were examined. Groth ('37) also investigated the mastoid region in primate skulls, and came to the conclusion that the mastoid process is a feature common to man and the great apes, and that in some respects its development is more marked in the latter. He points out that the manner of insertion of the sternocleidomastoid and *splenius capitis* muscles is the same in both, and in his view it is on the pull of the latter that the variable shape of the process mostly depends. Forster

('31) studied the form of the process in several species of mammals, including a few monkeys and apes, in relation to the insertion of these muscles, and in particular the sternocleidomastoid. He describes the mastoid process as an "ébauche" in certain of the monkeys, and as present in the apes. His own observations were insufficiently numerous to give any indication of the extent to which the process varies in size and shape within and between species.

The present paper provides more precise information about the frequency and size of the mastoid process in the gorilla and chimpanzee, based upon a study of some 300 adult skulls. This information helps to provide a background for evaluating the condition of the mastoid region in the Australopithecinae.

MATERIAL STUDIED

Forty-four male chimpanzee skulls, 88 female chimpanzee skulls, 96 male gorilla skulls, and 77 female gorilla skulls were studied in the Powell-Cotton Museum, Birchington, the British Museum of Natural History, London, and the Department of Anatomy, Birmingham University. Only skulls with a full permanent dentition were examined. The sexes of most of the chimpanzee and gorilla skulls in the Powell-Cotton collection were known from field records. The remaining skulls were sexed by a quantitative examination of the usual anatomical features.

RESULTS

The shape and size of the mastoid process varies considerably in both species of African ape. Sometimes it is completely absent; sometimes extraordinarily prominent. In shape it varies from a nipple-like projection to a large and projecting boss, as well developed as it may be in a massive human skull. As a rule it is more bulbous, and less compressed from side to side, than in man. Even when absent as a distinct mamillary projection, it is usually represented by a roughened prominence or ridge on the mastoid bone behind the ear. Where the process is small, the digastric notch sometimes seems to be on the projection. Where the process is well developed, a more typical notch and occipital groove occur, although it does not seem that the notch is ever quite as narrow and deep as it is in some human skulls. Correspondingly the medial lip of the notch is practically never as ridged as it is in man, and while the mastoid process when developed always has a free medial surface, this also is in our experience never as flat as it is in man. Broken specimens show that the process is honeycombed with air cells (fig. 1).

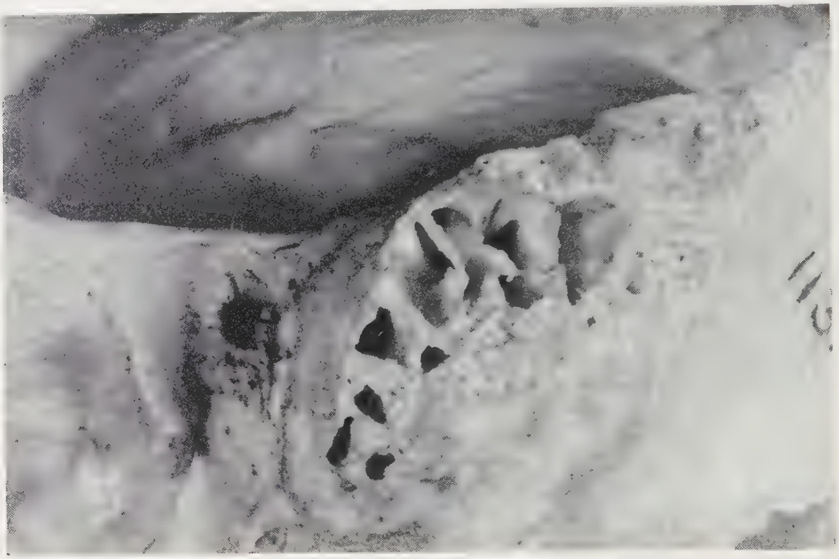

Fig. 1 The air cells of the mastoid process in the adult male gorilla.

The size of the process was classified in each sex of both species of great ape according to an arbitrary scale: Group 1: absent or slight (figs. 2, 3, 8, 9); group 2: intermediate (figs. 4, 5, 10, 11); group 3: prominent (figs. 6, 7, 12, 13). A free medial surface to the process could be defined both in groups 2 and 3. The results of this classification are set out in table 1, from which it is clear that a mastoid process of intermediate or prominent size is the rule rather than the exception in the African great apes. Relatively more gorillas than chimpanzees have a mastoid process and, correspondingly, the process is more frequently prominent in the gorilla. A χ^2 test of our figures showed that the difference in the frequency of prominent processes between the two species was statistically significant ($P \leq 0.02$) in the case of the males, but not in that of the females. Our figures also suggest that a prominent process occurs more frequently in the male than the female gorilla, a χ^2 test showing that this sexual difference is statistically significant ($P \leq 0.02$). Such sexual differences as our figures reveal in the prominence of the process in the chimpanzee were not, by the same criterion, statistically significant.

TABLE 1

The mastoid process in the chimpanzee and gorilla

	NO. OF SKULLS OBSERVED	PER CENT WITH MASTOID PROCESS CLASSIFIED AS		
		Absent or slight	Intermediate	Prominent
Male chimpanzee	44	36	18	45
Female chimpanzee	88	47	22	32
Male gorilla	96	13	15	73
Female gorilla	77	13	39	48

DISCUSSION

Descriptions have been provided of the shape of the mastoid region in some of the australopithecine fossils. Broom (Broom and Schepers, '46) in his first monograph writes that a prominent mastoid process is present in the type skull of *Plesianthropus transvaalensis* (S. 1), and that "This condition is quite unlike that in the chimpanzee, where there is usually no indication of a mastoid process and no digastric fossa except occasionally when it is represented by a slight groove. In the gorilla there is usually a well developed mastoid process, but no inner swelling and no very distinct digastric fossa." What appears to be another specimen of *Plesianthropus* (p. 53) is described as having a mastoid "much more like that of the chimpanzee than that of man." *Paranthropus robustus* is said to have "a mastoid process and a digastric fossa somewhat as in Plesianthropus, rather than a rounded mastoid structure such as we have in the chimpanzee. This it will be seen agrees much more closely with the condition in the chimpanzee than with that in man, and a little less closely with that in the gorilla.'' In his second monograph (Broom, Robinson and Schepers, '50), Broom states that *Plesianthropus* 5 had ''a small mastoid process,'' and that skull 8 had ''a fairly well developed mastoid process,'' much of which had been ''gnawed off before fossilisation.'' Some of Broom's views about the mastoid process and about the conformation of the mastoid bone in the great apes do not accord with our own observations, but his descriptions and illustrations of this region of the skull in the Australopithecinae suggest that its shape and prominence vary in these hominoids in much the same way as they do in the living African great apes. We have reached the same conclusion on the basis of an examination of casts of *Plesianthropus* and *Paranthropus* in the British Museum, and of some of the original material relating to *Paranthropus crassidens* which Mr. J. T. Robinson kindly allowed us to study. In our view this region of the skull, in the specimens we have seen, cannot be differentiated from that of the living African apes by any obvious characteristic.

We are much indebted to Mrs. H. B. Powell-Cotton, Birchington, for allowing us to study and photograph her collection of anthropoid skulls, and to Mr. T. C. S. Morrison-Scott for giving us access to the collections of chimpanzee and gorilla skulls in the British Museum. Expenses incurred in the fieldwork were met by a grant from the Wenner-Gren Foundation. Those arising in connection with the analysis of the data were met by a grant from the Royal Society of London. Our best thanks are due to Mr. W. J. Pardoe for preparing the photographs.

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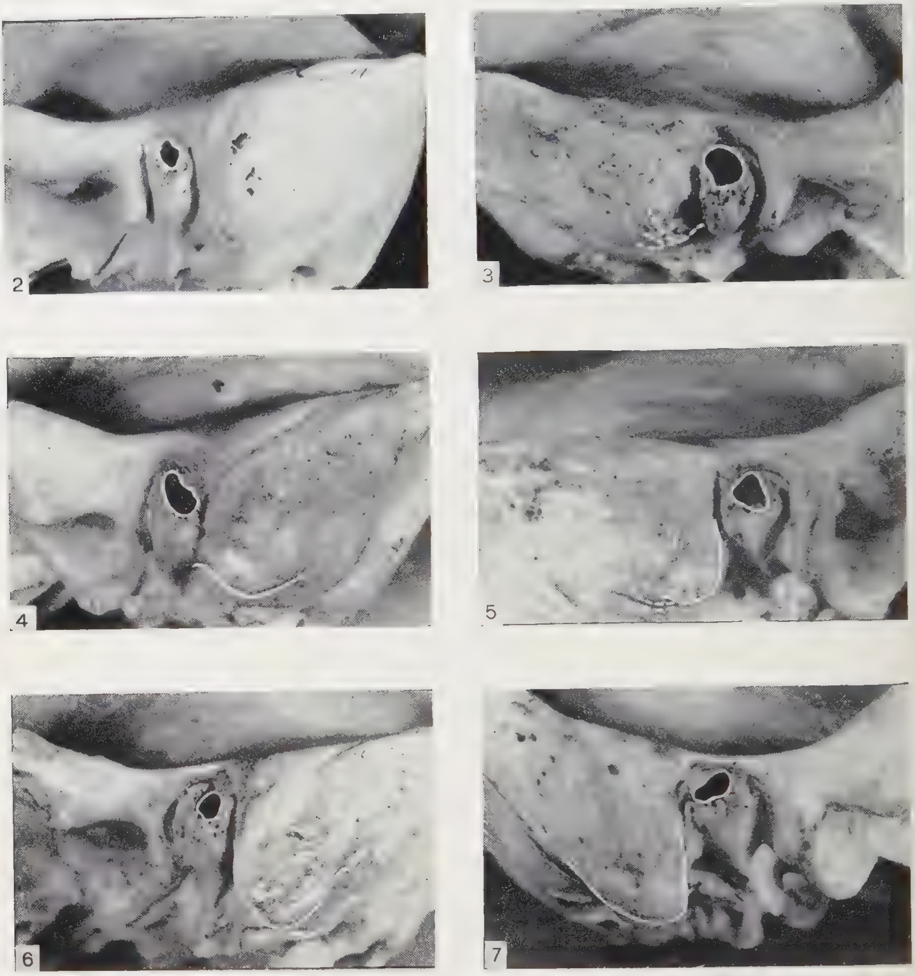
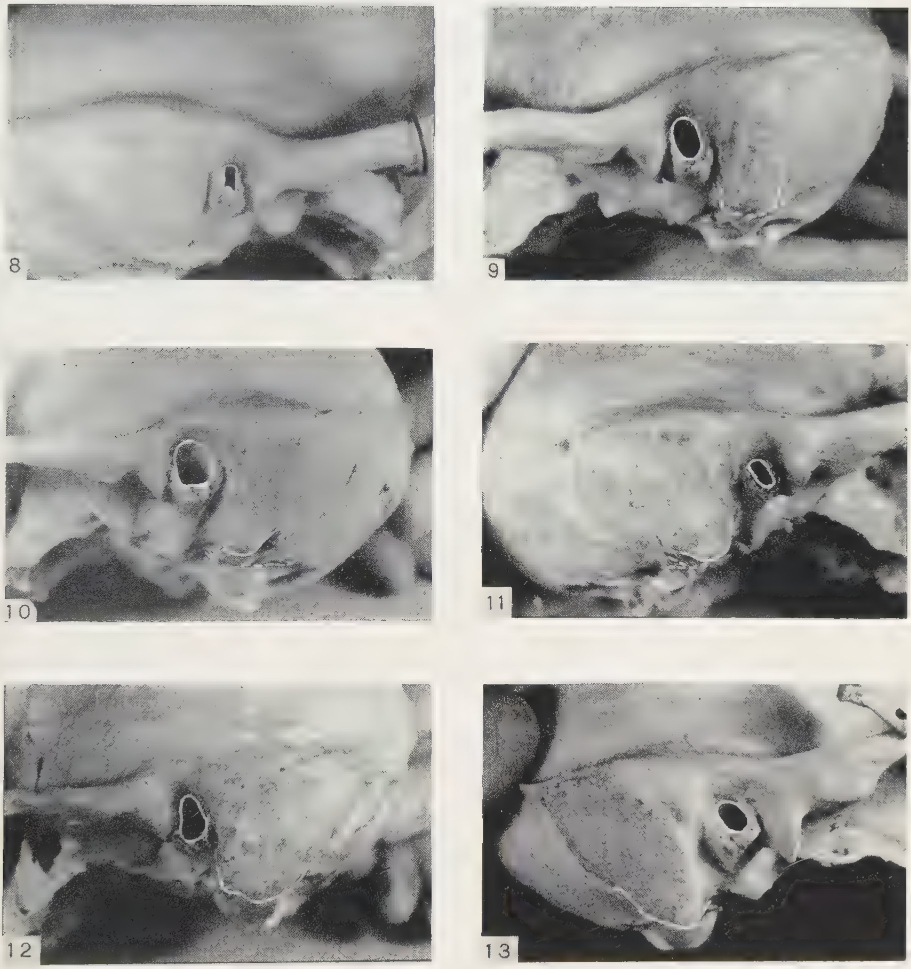


Figs. 2 to 7 Variation in the mastoid process of the adult male gorilla.



Figs. 8 to 13 Variation in the mastoid process of the adult female chimpanzee.

Distribution of Blood Groups in U. S.— Chosen for review are [ABO blood group distributions] pertaining to 141.774 white persons who voluntarily contributed blood to the American Red Cross in 15 cities and 637 outlying communities during the period January, 1948, through March, 1949. This is one of the largest aggregate samples ever described for a program under standardized technical procedures. . . .

For regions as a whole, the blood group percentages and corresponding standard deviations can be stated as follows: O, 45.55 ± 0.13; A, 40.77 ± 0.13; B, 9.96 ± 0.08; and AB, 3.72 ± 0.05. These over-all percentages differ in minor degree from those previosuly reported by Snyder, based on 20,000 persons in the United States. . . .

From region to region the greater the O, A or B percentage, the smaller on the average was the percentage for the remaining groups within the trio, but AB tended to be stable. . . .

For each degree of latitude proceeding from north to south the O percentage was greater by 0.32 on the average. To explain this tendency on the ground of adaptive variability, as understood in the genetic sense, would be unjustifiable. Probably the most acceptable interpretation is that in the South the hereditary lines of the early settlers, many of whom were Scotch, Irish and Welsh in origin, have been subjected to less admixture from the outside than in the North. . . .

From north to south the B percentage averaged 0.17 smaller per degree of latitude. . . . The A relationship, though significant ($r = 0.51 \pm 0.19$), is deemed of little importance quantitatively. George W. Hervey, Louis K. Diamond and Virginia Watson. Geographic blood group variability in the United States. J. Am. Med. Assn., vol. 145, no. 2, January 13, 1951, pp. 80–81.

MISSING TEETH WITH SPECIAL REFERENCE TO THE POPULATION OF TRISTAN DA CUNHA

SELMA Ø. THOMSEN
Forsyth Dental Infirmary for Children, Boston, Mass.

TWO FIGURES

Deviations from the usual number (32) of permanent teeth in man are known as hypo- and hypero-dontia. Hypodontia, or missing teeth, the more common anomaly of the two, is the sole concern of this paper.

Missing teeth have been studied from various points of view, such as etiology, location, racial differences in incidence and pattern, sex differences and mode of inheritance. While no attempt at a complete review of the literature is made here, a brief summary and discussion of these important aspects is desirable before presenting the data on missing teeth in the population of Tristan da Cunha.

PREVIOUS STUDIES

Etiology. Hypodontia is pre-eminently a human feature, rarely observed in subhuman species (Schultz, '34; Montagu, '40). In the older literature, its occurrence is often blamed on various pathological conditions, such as syphilis, tuberculosis, scarlet fever, rickets, vitamin deficiencies, and any other disturbance (e.g. endocrine) that might affect growing and developing tissues, including the dental ones. Interference with the development of dental anlages may well apply to incidental cases, but it does not appear plausible when an investigator attributes every missing tooth to the one particular disease to which his interest is confined. Campbell ('34), for example, believes that syphilis plays an important role in "most" cases of missing teeth. Wheelon ('25) and Hutton

('36) have related missing teeth and other dental disturbances to endocrine dysfunction. Hutton states that of all endocrine glands the thyroid is of greatest importance in dental development. Brekhus ('44) implicates endocrine dysfunction when teeth are missing in large numbers. Downs ('27) concurs with the opinion that dental anomalies frequently occur in individuals with endocrine dyscrasias, but doubts whether they can be "considered as pathognomonic of any definite type of endocrine dyscrasia."

Mental defectives as a group are also known to have a relatively high incidence of missing teeth (Cohen and Anderson, '31).

While the preceding discussion has pertained to teeth in general, we shall now consider *specific* teeth. Montagu ('36, '40) attributes absent or defective maxillary lateral incisors to their close relationship with the premaxillary-maxillary suture, an area which he apparently considers a "locus minoris resistentiae," easily disturbed by factors that might cause degeneration or variation in bone and neighboring teeth. Thus a degenerate or missing lateral incisor is considered an abnormality of the same nature as complete failure of closure of the bony suture (hare lip, cleft palate), differing from the latter only in degree. The lack of mandibular central incisors has been attributed to a similar proximity to the mandibular symphysis. Even localized inflammatory processes and traumata (extraction) are reported to be responsible for an incidental missing tooth.

More recently, however, attention has been focused on heredity as a possible etiologic factor, not necessarily associated with any pathologic conditions. Two exceptions should be mentioned. Ectodermal dysplasia, affecting tissues of ectodermal origin, like hair, nails, teeth, skin and skin glands, is often associated with a reduction of tooth number or even complete absence of teeth, and is known to be inherited (Thoma, '34). The other exception, also an hereditary syndrome, has been described recently by Böök ('50) and comprises hypodontism of the premolar region, hypohidrosis of the palms of hands and soles of feet, together with a premature whitening of the hair.

This brief review will suffice to demonstrate the great variety of presumed causes of missing teeth — several of which may in fact be valid — and consequently the necessity for distinguishing between the various categories of such teeth.

Terminology. From the above discussion, it becomes clear that terminology should imply cause, whenever possible. The best and most commonly used label, "congenital absence" or "lack of" teeth may cause confusion unless it is understood that at birth no teeth are normally visible in the oral cavity and few or none in the radiographs of the jaws. "Partial (or complete) anodontia" and "hypodontia," although frequently used, do not convey any clearer meaning than that teeth are missing. "Aplasia," connoting incomplete or faulty development, and "agenesis," failure of development of the dental anlage, cannot always be distinguished. Without verification of genetic traits, the designation "hereditary missing teeth" is misleading, but is commonly used when no other obvious explanation can be given. In general, it can be stated that the current terminology is unsatisfactory.

Incidence of missing teeth. 1. Deciduous dentition. The incidence of missing teeth is much lower in the deciduous than in the permanent dentition. This observation is not well documented, however, since very few reports deal with the deciduous dentition. Most data have been obtained on school children of more advanced dental development.

Werther and Rothenberg ('39) report only one child with missing deciduous teeth out of 1000 children studied. Röse ('06) compared the incidence of missing teeth in 7500 boys with deciduous dentition with that of a group of older boys with permanent teeth and found missing teeth much less frequent in the younger group.

2. Permanent dentition. It is generally agreed that reduction in the number of permanent teeth does not take place indiscriminately in the two dental arches, but is restricted to certain teeth. While this does not exclude the possibility that

any tooth may be missing, it does mean that certain teeth are more labile than others. The stability within each morphologic group of teeth decreases from mesial to distal. Thus the first premolar is more stable than the second, etc., the only exception being the mandibular incisors, where this process is reversed. This phenomenon of decreasing stability from mesial to distal has been explained by an extension of Butler's field concept ('39). Butler suggests that within each tooth group is a key tooth, manifesting the group character to a maximum degree. This tooth corresponds to the greatest strength of a "field of influence" governing size and form within the group; away from this, in either direction, the teeth in the group are less characteristic.

However, other hypotheses have been suggested. Schultz ('34) and others have pointed out that the evolutionary tendency toward reduction of the dental arch occurs at opposite ends of each quadrant, affecting the third molar and the smaller incisor of each jaw (the maxillary lateral and the mandibular central incisor). Others have offered the simple explanation that it is the smaller and presumably less resistant tooth of each group that is disappearing, yet they fail to point out that the mandibular P_2 is an exception to this rule.

The abnormality of missing teeth occurs in gradations from a slight size reduction of one tooth to complete absence of the whole group of two to 4 teeth. The second premolar, however, is rarely found abnormally reduced in size, but is often missing.

Since more than one group of teeth may be missing in the same individual, a study of the various combinations of missing teeth is important. This would reveal the possible existence of diverse patterns and also determine whether there is any regular sequence of vulnerability so that some teeth are always affected before others. The incidence of missing teeth is therefore of interest with respect to both location and frequency.

Racial differences. It has often been stated that reduction in number of teeth is further advanced in Whites than in other races. This premise is difficult to accept, since most studies which include large numbers of individuals and all groups of teeth deal with populations belonging to the White stock, whereas comparable studies of other races, Negroids in particular, are scarce. No accurate statement concerning racial differences can be made until more comparative data are forthcoming.

Numerous reports on the absence of single teeth, especially the third molar and the maxillary lateral incisor, give some indication of existing racial differences. For instance, studies of missing third molars have revealed a high incidence in various populations of Mongoloid stock (Goldstein, '32; Hellman, '36; Pedersen, '49), while in contrast the corresponding incidence in Negroids is reported to be very low (Hellman). In addition, Tratman ('40) has stated that Mongoloids have a relatively high incidence of missing mandibular incisors and a low incidence of missing maxillary lateral incisors, which proportion is reversed in Whites.

Within the White race, population differences are also reported, some of which are misleading because they reflect the influence of small samples, inaccurate observation or differences in examination technique or methods of presenting data. A good example of the extreme variation reported in incidence of missing third molars in Whites is furnished by Hellman's findings of 49% among Hungarians and Brekhus ('44) of 0.6% in a Minnesota population.

A smaller but still considerable range of variation is found for the maxillary lateral incisor, missing and "degenerate" in 5.2% of Whites according to Hrdlička ('21) and in 0.9% according to Werther and Rothenberg ('39).

One may summarize the state of knowledge in this field by saying that real differences have frequently been overlooked, with many investigators doubting the validity of findings different from their own. It should be remembered that populations belonging to the same racial subgroup may still differ in gene frequency, a fact which may well explain marked variations in incidence of missing teeth.

Missing teeth and skull form. Röse ('06) relates the incidence of missing teeth to skull form and thereby explains interpopulation differences. After examining 12,000 males of various populations of North and Middle European origin, he found dolichocephalic populations to have more missing maxillary lateral incisors than brachycephalic populations, while the latter had more missing third molars than the former. He found a similar relationship between missing teeth and skull form also *within* these same populations; the latter point is of great importance but usually overlooked by later investigators. As an explanation for these relationships, Röse suggested the hypothesis that the growth of the "Zahnleiste" is impaired anteriorly in a narrow skull and posteriorly in a short skull. Röse detected no relation between facial index and incidence of missing teeth, nor between the latter and the space available in the jaw. This last point has been confirmed by others, such as Pedersen for the Eskimo dentition.

Röse's work has received much attention from other investigators, notably Dolder ('36), Hjelmman ('28) and Pedersen, in regard to the incidence of missing teeth and the relation of missing teeth to skull form, but with discordant results. Perhaps these can be attributed to the different racial stocks involved. Röse's findings may not hold universally; indeed, Muller ('49) has called attention to the fact that the same character may behave differently in different races.

Sex differences and bilaterality. Reports in the literature suggest that a sex difference in incidence of missing teeth exists, with females more affected than males, but the evidence is not clear. Similarly, differences in laterality are not easily evaluated and may well be due to chance.

Genetic aspects of missing teeth. It has been shown in numerous reports, many of which are reviewed by Gates ('46), that missing teeth can be genetically determined. Yet the problem is not fully understood. Iltis ('48) in reviewing the literature states that every mode of inheritance has been suggested. Table 1 shows the great diversity of conclusions reached by various investigators concerning the mode of inheritance of missing maxillary incisors. Most of the authors listed in this table base their conclusions on pedigrees of one or more generations.

Böök states that most instances of missing teeth are due to autosomal dominant genes. He considers the frequently stated concept of an evolutionary tendency toward reduction of number of teeth as "obscure" and assumes that "most cases of hypodontia in man are originally due to mutations." Böök realizes the complexity of the mechanism of inheritance. He states, "Not only is hypodontia caused by different genes with varying degrees of penetrance in different pedigrees, but aplasia of a special tooth group or even individual teeth may be transmitted in one family as an autosomal dominant and in another as a recessive or sex-linked trait." Thus it can be seen that there are many conflicting opinions on the subject of mode of inheritance of missing teeth.

TABLE 1

Diverse modes of inheritance attributed to missing maxillary incisors. All studies concern the lateral incisors except for Huskins' on central incisors

AUTHOR	MODE
Jöhr	Dominant
Wheelon	Dominant
Schultz	Dominant, more frequent in the female
Snyder	Dominant, sex-linked
Keeler and Short	Irregular dominant
Iltis	Recessive
Thomsen	Recessive
Huskins	Partial dominant, sex-linked with variability in penetrance and expressivity

PRESENT STUDY

The present study is based on material collected by Dr. R. F. Sognnaes during the Norwegian Scientific Expedition to Tristan da Cunha in 1937–38. This material consists of dental casts, intraoral roentgenograms, and complete genealogies dating back to the first settlers.

Tristan da Cunha is a group of 5 small islands, of which only one is inhabited, situated in the middle of the South Atlantic Ocean, approximately at 37° S and 12° W. At the time of the Norwegian expedition in 1938, the population consisted of 188 individuals, descended from 8 men and 7 women who settled in Tristan at different times from 1816 on. All the male ancestors and two of the females were White, whereas 5 females are reported to have been "of mixed European and coloured origin." It is not known whether the colored part was Negroid, Malayan, or both. Four of the colored females were born on the island of St. Helena, two being sisters, and a third a daughter of one of these sisters (Munch, '47). The Tristanites are thus a product of several generations of inbreeding and since they have led an isolated existence, with all matings known, they constitute desirable material for a genetic study.

The incidence of missing teeth in the Tristanites was appraised from dental casts and roentgenograms, excluding those subjects for whom no reliable evidence was available. After this selection the third molar could be studied in 94 individuals the second premolar in 158, the maxillary lateral incisor in 140 and the mandibular incisors in 169 individuals. Although the total population was not included, findings on the Tristanites who are actually studied probably approximate the true incidence of missing teeth in each tooth group.

Missing teeth in the Tristanites occur only in certain pedigrees, and since no evidence exists of any other condition that might cause absence of teeth in the Tristanites, it may be concluded that the trait is genetically determined in this population.

The deciduous dentition was not affected, because in all individual cases the absence of deciduous teeth could be accounted for by exfoliation, according to Sognnaes.

Incidence. In the permanent dentition, only the teeth generally regarded as "unstable" are missing (with the exception of mandibular I_2) and in the following rank order (fig. 1): the third molars in 19% of the subjects studied, the second premolars in 7%, the maxillary lateral incisor in 0.7% (pegshaped in 5%), and the mandibular incisors in 2% (I_1 in 1%; I_2 in 1%).

One or more teeth are missing in 18% of all the Tristanites with permanent dentitions. The true figure may be even larger, since not all groups of teeth could be studied in the total of 169 individuals. When expressed as a percentage of the number of teeth, the third molar is missing in 21%, the second premolar in 7%, the maxillary lateral incisor (pegshaped or missing) in 3% — and the mandibulary incisors missing in 0.6% of the respective numbers of teeth studied.

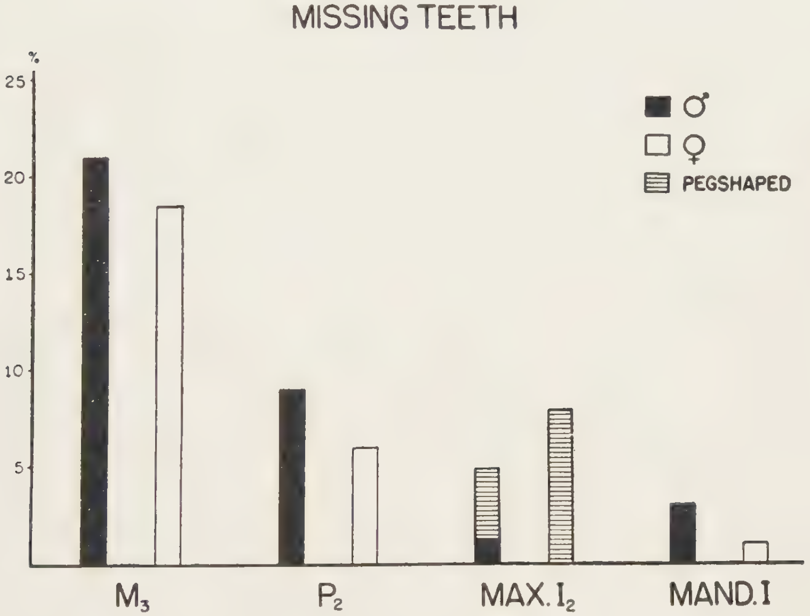


Fig. 1 Incidence of missing teeth in the population of Tristan da Cunha expressed as a percentage of individuals studied.

There are no statistically significant differences in the occurrence of missing teeth as between the two sexes, the two jaws, and right and left sides.

A study of the combinations of various groups of missing teeth in the same individual shows that in 74%, or 13, of the

18 individuals in whom third molars are missing, no other class of teeth is missing. It is difficult to make any definite statement regarding the other groups of missing teeth because the status of the third molar could not always be clarified in those individuals who were missing other teeth. However, all possible combinations of missing teeth can be demonstrated in this population. Thus it is seen that any of the reported missing teeth can be absent either alone or in association with its antimere or with the corresponding tooth in the opposite jaw. Furthermore, one group of teeth can be missing regardless of the presence or absence of any other group of teeth.

Rank order. The rank order of groups of missing teeth in the Tristanites is similar to that found in other studies, insofar as the absence of M_3 is concerned, but differs in that the maxillary lateral incisor in the Tristanites is less often missing than any other tooth. This last mentioned finding may be due to chance.

Genetic aspects. The genetic aspects were studied from complete genealogies dating back to the first settlers of Tristan da Cunha in 1816. The population of the island in 1938 consisted of 188 individuals representing three generations but, unfortunately, many of the children were too young to show any evidence of permanent teeth even in the roentgenograms, and had to be excluded from the study. The trait of missing teeth has been observed in several pedigrees (fig. 2) and behaves in most cases as a Mendelian recessive.

Comparison with other studies. In comparing the incidence of missing teeth of Tristanites with that found for other populations, one must take into consideration the fact that the Tristanites are highly inbred. The incidence of this genetic trait must, therefore, be expected to be higher than in a group taken at random out of a large population. Jöhr's ('34) study of missing maxillary lateral incisors in an inbred Swiss population shows an even greater incidence (33% of the populace) than that found in the Tristanites. However, Jöhr's data pertain *only* to the maxillary lateral incisors. Most other studies similarly consider only one morphologic class of teeth (M_3 for instance), or else exclude one group of teeth while dealing with all others. The few available reports in which all tooth groups were included disclose that 1 to 5% of the individuals studied have one or more missing teeth. The incidence of missing teeth in the Tristanites (18%) is therefore relatively high.

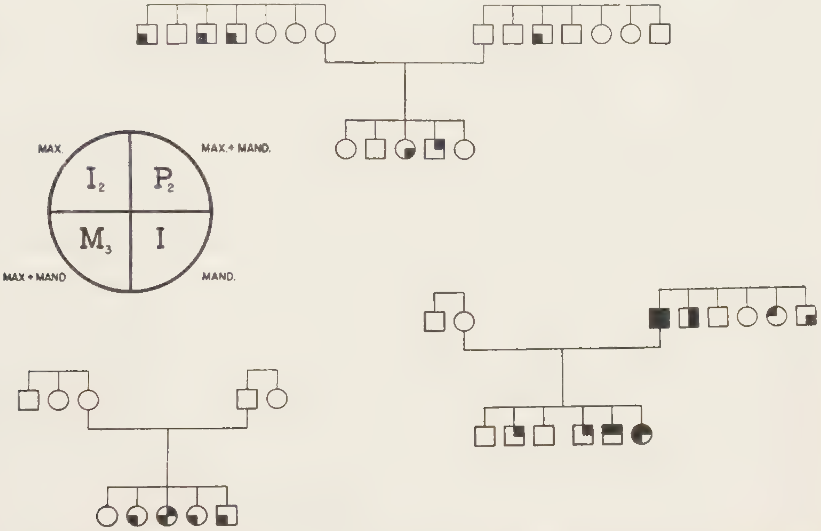


Fig. 2 Genealogies of three Tristanite families missing various groups of teeth. The teeth missing are shown by blacking out the appropriate sections as indicated by the key.

Conclusion. From this and other studies it may be concluded that the incidence of missing teeth varies from population to population. Some of the apparently marked differences may be spurious. On the other hand, we may expect differences in gene frequency and mode of inheritance of missing teeth from one population to another, and some of the differences noted in incidence of missing teeth may be thus explained. The very high incidence of missing teeth among the Tristanites is most readily explained in terms of the genetic mechanism of inbreeding.

SUMMARY

1. The literature on missing teeth is briefly reviewed.

2. Among 169 inbred inhabitants of Tristan da Cunha, primarily White with some "colored" admixture, the incidence of missing teeth is 18%. This compares to 1 to 5% in non-inbred Whites and 35% in a highly inbred Swiss group.

3. The rank order of missing teeth is M_3, P_2, mandibular I_1 and I_2, and maxillary I_2.

4. The trait behaves in most cases as a Mendelian recessive.

ACKNOWLEDGMENTS

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Exhumation vs. Excavation of Cemeteries.— During 1950 the United States Steel Corporation purchased a large tract of land in Lower Bucks County [Pennsylvania] for construction of a steel plant, and a large portion of the Penn Manor in Falls Township was included in this tract. As a condition of the land transfer, the steel company agreed to move the Falls Meeting House Cemetery and several farm burial grounds to the Falsington Meeting House Cemetery. One of these small plots was of some interest because it was the Harrison-Pemberton cemetery, closely associated with William Penn and his country home, Pennsbury. James Harrison, Penn's friend and steward and the man who built Pennsbury, had his final resting place here among the members of his immediate family. It was therefore decided that this cemetery should be moved to an appropriate place on the grounds at Pennsbury, which is a property of the Commonwealth of Pennsylvania, administered by the Pennsylvania Historical and Museum Commission. . . .

The moving of cemeteries on the steel company tract was let on bid to an undertaker, Arthur Hough, of Trenton, New Jersey, with a contract with a time clause. The Pemberton plot was included in this contract prior to the decision to remove it to Pennsbury. I was present during the exhumation as an agent of the Commission, and Hough and I directed the excavation. Since the problem of identification of individual graves seemed important here, the area was stripped and worked out in much the same fashion as any archaeological site. . . . The extremely elaborate legal arrangement, the detailed businessman's conditions of the contract, and the careful planning of the new cemetery by a first-rate period architect contrast strongly with the perfunctory arrangements for the excavation. Actually a site of this type, located in these particular soil series presents one of the most difficult field problems in local archeology. The moving of cemeteries on valuable industrial and urban land is only now becoming a common problem, and it is invariably handled on a bid and contract basis, in which the work is done entirely by morticians. The difference between an ordinary exhumation and the excavation of a site like this is tremendous, and under no circumstances should the latter be handled by such a contract.— John Witthoft. The Pemberton Family Cemetery. Pennsylvania Archaeologist, vol. 21, no. 1–2, January-June, 1951, pp. 21–32.

PHYSICAL GROWTH AND DEVELOPMENT

STANLEY MARION GARN
Forsyth Dental Infirmary for Children, Boston

THE MEANING OF GROWTH AND DEVELOPMENT

The terms ''growth'' and ''development,'' as used in physical growth studies, refer to processes common to all living organisms, processes intimately linked in time, but partially independent, unquestionably genetically determined, yet uniquely susceptible to environmental modification. These two processes, growth and development, bring the individual from the fertilized ovum to final size, and thus constitute the link between the invisible gene and the visible phenotype. And because growth and development are processes, research in this field can disclose not only differences and similarities that exist, but also some explanation of how they came to be.

''Growth'' refers mainly to changes in magnitude, increments in the size of organs, increases in the thickness of tissues, or changes in the size of individuals as a whole. Growth may be defined in terms of change (increment), rate (increment per unit time), the momentary rate of change, or the pattern of changes in the growth rate throughout life. Since each individual has his own total growth pattern, the sum of the growth patterns of his component parts, growth may be investigated in two ways: as segmental and regional growth — the growth of the parts, and as total growth — the growth of the individual as a whole. Naturally, when environmental changes are under investigation, these two approaches together help to show whether the modification of size or form is primarily a matter of over-all change, or alterations in the rate or pattern of regional growth.

''Development'' is closely related to growth, but development can take place without changes in magnitude (just as growth can take place without development). Development implies differentiation (as during the first trimester), changes in proportions (as during the steroid growth phase) and changes in complexity (as characterized by neural development). Development may be due to a constant difference in growth rates, so that one tissue consistently increases faster than the other, or to differentials following temporary stimulation or sudden inhibition by a hormone or a growth antagonist. Development may be evaluated in terms of one organ or one tissue alone, or in terms of the organism as a whole. And environmental changes may affect over-all development, or they may stimulate one organ or tissue, and barely affect the rest.

Individuals differ in the rate and pattern of growth, and in the rate and sequence of development. Such differences exist between male and female, between individuals of different races and geographic groups, and undoubtedly between recent and fossil men. Partly for our own information we must determine what differences in rate and sequence are primarily genetic, which hormonal and neurohumoral factors mediate the inherent pattern, and what environmental factors can and do modify pattern, rate, and sequence. And, in order to explain evolutionary changes, we must acquire some understanding of the functional significance of these differences in growth and development.

MATERIAL FOR THE STUDY OF GROWTH AND DEVELOPMENT

In the investigation of growth and development, we are primarily interested in growing organisms, and the changes in these organisms, whether they represent normal growth, deviations from the usual pattern, frankly pathological manifestations, or the results of deliberate experimentation. Necessarily, during the initial phase our chances for observation are limited. We cannot peer into the gravid human uterus, a limitation only partially offset by in-vitro studies, embryos secured through surgical intervention, studies on laboratory animals, and by clues furnished by full-term infants with developmental defects. Beyond the first two months of pregnancy, available material increases: still-births (often pathological), premature births (or fetal infants as Gesell chooses to call them) and full-term but developmentally-immature infants are all available for study.

Despite the relative inaccessibility of the viable human fetus, it is still possible to make controlled studies of prenatal development. By selection of pregnant women, with careful attention to race, socio-economic status, mode of life, diet, and age, many factors affecting prenatal growth can be isolated. Naturally, in the neonatal period, more sophisticated experimental designs are possible. With the infant before us, we can deliberately vary the amount and proportion of nutrients, the size and number of individual feedings, and the temperature and humidity as well: we can investigate growth first-hand. Many interesting problems, including birth size, the significance of birth order, sexual and racial differences at birth, and the relationship between body size and developmental status, can be investigated through studies on the neonate, by similar studies on older children, and even young adults as well.

Orphanages, homes for underprivileged children, and similar institutions, make possible controlled experiments where it is practical to alter only one variable at a time, while maintaining both test subjects and controls in the same uniform environment. Though we would not care to subject young children to low-protein and meat-free diets, there are religious and cultural groups that abstain from meat and other animal products (milk, eggs, and cheese), and their children do too, once they are weaned. So it is possible to investigate the influence of such unusual diets upon child growth. Finally, we should not forget that an army of occupation may be paternalistic in a biological sense, fathering examples of race mixture, which when studied in an institutional context, can help to establish the existence of racial differences in human growth and development.

While most growth studies follow only "normal," clinically-healthy children, pathological states should not be ignored, and they can contribute much to our knowledge of growth and development. For example, the effect of various hormones on human growth and development cannot be determined entirely from animal studies. Species differences are great: generalizations true for beagles or hamsters may not apply to humans. Experimentally induced hypothyroidism, hypoparathyroidism, or panhypopituitarism is ordinarily out of the question in man. But, by following children with known endocrinopathies, before and during treatment, the growth-regulating effects of these hormones can be investigated. Bone dystrophies are also informative. In many dyschondroplasias, such as Morquio's syndrome and Silfverskiold's syndrome, marked regional autonomies are found. For example, in Morquio's syndrome the growth of the head is but slightly inhibited, while the long-bones are warped, stunted and poorly calcified. In Silfverskiold's syndrome the lower leg is dwarfed, while the rest of the patient is normal, an observation in line with other evidence that leg growth may be influenced by genetic factors that do not affect the rest of the body. The extent to which bone form is determined by inherent factors, or moulded by musculature, can be found through the study of unilateral defects such as hemiplegia, unilateral paralyses, ankyloses and the like. Given a child with a totally paralyzed leg on one side, and an unaffected leg on the other, differences due to nutrition and age are largely eliminated, and only the pertinent differences remain: in short, one side serves as a control for the other.

It should be stressed at this point that successful growth studies depend upon the proper selection of material for investigation, and definite goals in mind. The investigation of sub-adult individuals does not in itself constitute a growth study. The mere application of anthropometric technique to children, or the taking of carpal x-rays or dental impressions of adolescents does not make a study relate to growth. A true growth study employs growing material, its methods are specifically designed to investigate growth, and its primary purpose is to investigate problems of growth and development.

GROWTH, DEVELOPMENT AND PHYSICAL ANTHROPOLOGY

In the study of growth and development, we are concerned with those individuals who are actively increasing in size, or who have not yet reached the "mature" steady-state of performance. This includes about 35% to 50% of the population, depending upon the population profile. Moreover, we are concerned with the origin and development of morphological characteristics. Some may be present only at birth and disappear at maturity. Others may be present from birth on, while still others may appear under the stimulation of adrenal or gonadal hormones. From a practical point of view, physical anthropology cannot neglect over half of the population, nor can it neglect the ontogenetic aspects of characteristics that are employed in human taxonomy, characteristics that can be used in human genetics, or that may be helpful in population studies. From these considerations alone, it is evident that the investigation of growth and development is an indispensable part of contemporary physical anthropology.

In human taxonomy we are interested in those traits that are genetically determined, and less in those that are environmentally produced. Metrical characteristics, once thought to be immutable, have been discarded as prime criteria of race, for a variety of studies since 1914 have shown that size and form alone are alterable in man, as they are in other domesticated animals. On the other hand, in growth studies we often encounter interesting differences that persist from birth on, and that are not primarily environmentally determined. For example, carpal development is more advanced in the Negro neonate than in the newborn White (Christie, '49; Dunham et al., '39) despite the smaller size of Negro babies, and the possibly poorer state of maternal nutrition. The relatively longer limb of the Negro is a characteristic that appears early in fetal development (Schultz, '26), it is marked during the neonatal period where the rate of limb growth in Negro infants exceeds that in Whites (Kelly and Reynolds, '47), and comparable racial differences in size of limbs are noted throughout the growing period (Meredith, '47). While we have been accustomed to attribute the smaller size of Negro infants to economic factors, their smaller size is not associated with the expected developmental retardation. On the contrary, osseous development is advanced (as just mentioned), and dental maturation is also advanced. In general, Whites erupt their permanent teeth later than non-Whites (Hellman, '23; Steggerda and Hill, '42; Garn and Moorrees, '51). Racial differences in immunity also appear to exist independently of dietary factors: Little ('43) has shown that Anglo-Negro children (the offspring of Negro fathers and British White mothers) have less dental decay than all-British children in the same areas, despite the standardized wartime diet common to both groups.

Studies in growth and development help to illuminate problems in body-build or physique, as well as problems in race. While it has been shown that quantitative and qualitative changes in diet do affect body form and size, genetically-related children may preserve similarities in body-build and proportions, despite marked differences in nutritional status and general health (Garn and Moorrees, '51). Moreover, longitudinal growth studies, like those at the Fels Research Institute and the Harvard School of Public Health, show that individual differences in tissue components are rather constant, and that children hold the same rank-order over a fairly long period of time (Stuart and Sobel, '46; Reynolds, '44, p. 199). Moreover, there is little doubt that body-build and the rate of maturation are inseparably linked. Linearity (ectomorphy) is definitely associated with late maturation (Jones, '49). Greater mass, especially muscle mass, is definitely associated with early maturation (Reynolds, '46; Jones, '49), and not just because earlier maturing children are necessarily heavier at age 12 or 13 than later-maturing children. The differences persist, and are reflected by the familiar inverse relationship between muscularity and stature in adults.

Turning now to hominid evolution, we must admit that little has been done to apply our factual knowledge of physical growth and maturation to the explanation of the pattern and course of human phylogeny. In part this is due to the fact that the subject of fossil man is ordinarily approached through the morphological comparison of adult specimens. But there were baby Neanderthaloids. We have fossil children at our disposal, and considering known differences in proportions and growth rates in contemporary children it may be possible to estimate whether the paleanthropic pattern of growth and development really falls outside of the present range. There is even more direct evidence: in many cases we are able to determine the sequence of tooth eruption, and having determined the sequence we can say whether or not it differs from the more common hominid pattern (Broom and Robinson, '51). It is true that we cannot determine the chronological age of fossil children, but by comparing osseous and skeletal development, and the relationship between endocranial volume and skeletal maturation, we can reconstruct some of the pattern and sequence of development.

In speculating about human evolution, we have stressed climatic changes that could have directed its course, to the relative exclusion of societal changes. For example, we have considered the possibility that increasing aridity pushed our progenitors onto the savannah, and thus selected for those who were ground-walking. We have considered Neanderthal man as a possible evolutionary product of the cold, treeless tundra. But it is likely that the evolution of culture affected the physical evolution of man just as profoundly as did climatic variations.

Consider contemporary man born in a high state of physical immaturity (as compared with the young of most species), and dependent on outside sources for thermal regulation: there is no built-in marsupial pouch. Without fur, and unable to perambulate for over a year, the human infant needs continued protection. He cannot forage for himself for over 6 years, and even in the friendliest climes must be ''minded'' for over a decade. Final size is not attained for over 15 years, well after the second molars emerge. This long period of slow growth and long dependency is unique, even for anthropoids, and it is hardly likely that it could have become established except in a group possessing some techniques of child care, some rudiments of culture. Given culture, late, slow maturation could have become established. Without culture, it is unlikely that individuals with such a growth pattern would survive.

But just as only the development of culture could make long, slow growth possible, consider the advantage of this pattern (early neural development, late maturation) to the acquisition and transmission of culture. The human infant is able to understand words before he is able to toddle. He can speak long before he is able to play with others. By the time that he can interact with other children he has been influenced by the culture. Before he is physically strong enough to be a serious threat to others, he has already learned most of the fundamentals of his culture. With early cerebral development and late physical maturation, he can be influenced to the greatest degree by the customs of his group. He learns by precept, experience and admonition, and finally when he is sexually mature, when he is a potential threat to group solidarity, the lessons have been learned. What has been learned is not only the technique of conforming with the group, but also the fear of not conforming, not only the fundamental techniques of survival, but also the mores, the customs, the values and the attitudes of the group. Compress the time spent in maturation, make cerebral growth relatively slower than it now is, and the acquisition and transmission of culture would not be possible.

PROBLEMS IN GROWTH AND DEVELOPMENT

Problems in growth and development vary with the developmental stage: what may be important at one phase of growth may not constitute a problem at another. During the first trimester, with all of the possibilities of developmental defects, our concern is centered on teratology, and the prevention of anomalies of development. Then, with such dangers over, the emphasis shifts to the maintenance of the embryo and the prevention of premature births: maternal health and prenatal nutrition are now of primary interest. In the neonatal period there are not only the hazards and difficulties of birth, the effects of isoimmunization, and the limitations set by developmental immaturity, but there are problems of nutrition, and thermo-regulation. Then, once the rhythm of sleeping and eating is established, our attention shifts to the pattern of growth, to sexual, racial and individual differences in development, and to the environmental modification of the rate and regularity of growth. Though problems in human growth and development are not exclusively medically-oriented, it is obvious that they are likely to bear upon areas of clinical interest.

Developmental defects are common. Perhaps 25% of conceptions fail to reach term because of failures in development. Over 1% of neonates exhibit developmental anomalies, and while serious defects (mongolism, microphthalmia, and aniridia) are relatively rare; cleft-palate, hare-lip, and limb abnormalities are of considerable practical importance as well as academic concern. Undoubtedly heredity accounts for a fair proportion of infants born with cleft-palate, symphalangism, or polydactyly (Reed, '49), but it is impossible to attribute all such cases to single-gene substitutions, and there is good reason to believe that certain of the more common congenital defects, such as monogolism, are not *primarily* determined by genetic factors (Benda, '49; Böök and Reed, '50). It has long been known that noxious stimuli of many kinds, magnesium salts, lead compounds, iodine and insulin, can cause developmental arrests if applied to the embryo during the critical early stage of development. Both the older and the more recent investigations show that the nature of the injurious stimulus is of less importance than the time of its application. Chemical, mechanical or thermal changes may arrest development, and thus affect those tissues or organs in the process of formation and rapid cellular multiplication.[1]

While it is clear that a variety of chemicals, as well as surgical intervention and elevated temperatures can lead to developmental defects, it is the generalization rather than the details that apply to man. Chlorobutanol, ether and chloroform can cause such anomalies in laboratory experiments, but ordinarily the pregnant woman is not exposed to such substances in quantities sufficient to injure the fetus. While alcohol can harm the fetal nervous tissue, even the most immoderate swiller of gin cannot build up enough ethanol in her blood to injure the embryo. At the present time only lead (possibly selenium and beryllium) are recognized as likely causes of developmental defects, and only under rather rare circumstances, where industrial health measures are lacking.

But metallic salts, surgical interference with the placental circulation, and thermal elevation all injure the embryo in the same way: they depress its oxygen uptake, and thereby cause anoxia. Since the most rapidly developing tissues necessarily demand the most oxygen, it is easy to see why oxygen deprivation can result in developmental defects. Now anoxia can also result from defective implantation or placentation. Tubal pregnancies, placenta praevia, and premature separation of the placenta all can interfere with the fetal oxygen uptake. There is a known relationship between defects of implantation and placentation and abnormal offspring in man. In mothers who have produced mongoloid offspring, such defects are unusually common (Ingalls, '47) and whenever pregnancies terminating in defective infants are reviewed, hemorrhages during the second month, infections, attempted abortion, uterine abnormalities, and abnormalities of placentation are disproportionately frequent (Ingalls, '47; Ingalls and Davies, '47).

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[1] Rubella (rubeola or German measles) during the first trimester may also be responsible for developmental defects. Retrolental fibroplasia, which is restricted to prematurely born individuals, is a developmental defect of unknown etiology, and is probably more common in the North.

Dietary deficiencies, as a cause of developmental arrests, should not be neglected. Domestic animals maintained on diets deficient in a single nutrient yield a high incidence of abnormal offspring. Rats on a rachitogenic or vitamin A-deficient diet, produce litters exhibiting a wide range of osseous defects (Warkany and Nelson, '40; Warkany, '44). While these generalizations may not apply directly to man, in that human populations are rarely deficient in only one nutrient, dietary deficiencies do exist, and it is probable that diets favoring developmental defects are more common in our "civilized" world than in preliterate societies. According to Burks and her associates ('43), mothers on "poor" diets produce more defective offspring than when the maternal diet may be rated as "good," in terms of present nutritional knowledge. Other dietary deficiencies have been investigated (with somewhat similar findings), but as might be expected, the epidemiology of developmental defects and dietary deficiencies is still largely unstudied in most cultures.

While it is axiomatic that maternal diet is important in the last two trimesters, as well as during the first trimester, it is still difficult to say just how important. Is the fetus parasitic, independent of the maternal food intake, or is its growth highly dependent upon the caloric consumption of the mother? Present evidence from two separate starvation areas (Stalingrad during the siege of 1942, and Holland during the "terrible winter" of 1944–45) shows that a slight decrease in maternal food consumption has little effect on the size of the infant at term. A marked decrease (down to starvation levels) affects the body length of the infant only a little, while body weight is considerably reduced (Smith, '47; Antonov, '47). In other words, length continues to increase despite the reduced maternal diet. Body fat, on the other hand, depends upon the mother's food intake. One factor (length) is independent; the other factor (fat) is dependent upon the caloric consumption of the mother. This finding is of clinical value, and of importance to those who must evaluate birth size in different racial and national groups.

Now a certain proportion of infants are born "prematurely," that is, they are developmentally immature, below 3200 gm in weight, or born well before the modal age of 280 menstrual days. Such infants are of interest not only because of the unusual care they require, but also because they pass through a phase of growth and development ordinarily hidden from our view. "Premies" are a problem because their enzymes, reflexes, and their thermo-regulatory mechanisms are still incomplete. We can supply heat and humidity, we can feed by dropper or tube, and we can provide special foods that are easier to digest. With such aid the larger "premies" (those well over 2000 gm) have a good chance of survival. If they survive, with modern care there may be no lasting retardation, except for that imposed by continued parental solicitude. But the smallest and most incomplete of these infants still have a poor chance of survival, less than 1 in 10 (Dunham, '48, p. 42). We do not yet know enough about life on the inside to help these venturers survive on the outside.

The full-term infant presents a number of physiological problems (Smith, '46) and, more along our line of interest, there is a host of physical paradoxes as well. The female universally averages smaller and lighter in weight at birth (Krogman, '41), and some authorities have even suggested that there is a shorter gestation period for the female fetus. Yet skeletally the female is more advanced at birth (Christie, '49) and she retains this advanced status throughout the growing period (Todd, '37; Greulich and Pyle, '50). She adds body fat more rapidly, and keeps it. Her teeth emerge earlier (Hurme, '49) and she is earlier in sexual maturation. Why these sex differences exist is not known, and though the pituitary body is probably larger in the immature female, a causal relationship need not exist. For that matter, why are the teeth of females smaller than those of males (Moorrees, '51)? What determines these pre-pubertal sex differences?

Body size in infancy requires more attention that it has yet received. We know that body weight can be quadrupled during the first year, and in some cases it has increased nearly 10-fold (Dunham, '48, p. 84). There seems to be a relationship between birth size and age at walking. Norval ('47) has shown an inverse correlation between body length at birth and age at walking. Presumably longer (but not necessarily heavier) neonates are advanced in other respects. How long the comparative advancement lasts has not yet been explored sufficiently and the whole question of racial and cultural differences in the rate of development remains open for investigation, rather than for further rationalization.

Change in body size, weight and skeletal status during infancy are to a large extent a matter of quantitative and qualitative differences in nutrition. In years past, when cow's milk was often the vector of disease, and modified, homogenized, and allergen-free milks were unknown, breast-fed babies gained more rapidly, and had an optimum chance to survive. At the present time the situation is very different. In many parts of the United States only a minority of infants have ever been breast-fed (Bain, '48), and formulas with added nutrients (and artificially-contrived low curd tension) afford the infant an opportunity to make full use of the notably higher calcium content of cow's milk. It is very difficult to prove that breast feeding is even psychologically preferable, and until such studies include adequate numbers, definite, incontestable conclusions cannot be drawn.[2]

[2] Without entering into the breast-feeding controversy directly, one may note that Freud in his later pronouncements on the subject, concluded that "it makes no difference whether a child has really suckled at the breast or has been brought up on the bottle and never enjoyed the tenderness of a mother's care." (Freud, '49, p. 90.)

We can now pump more nutrients into infants than ever before. With ergosterol, dextrose, lactose, amino acids and meat by-products, babies become fatter, bigger and more advanced skeletally. The rate-of-gain during the first year has been doubled. Unfortunately, we have too little data (despite a multiplicity of studies) to say whether long-bone growth has increased at the same rate as body fat. But it is more than likely that the A and D vitamins, added calcium, and more amino acids have speeded up femoral and tibial growth, as well as increasing infantile plumpness.

One also wonders whether the altered pattern of infant nutrition may account for the well-known but poorly understood secular change in stature that has been demonstrated by a variety of workers. The phenomenon itself is undeniable. It is true in America, Europe and Asia (Bowles, '32). The legs and arms appear to have gained more than the torso (Bowles, '32, pp. 49–52; Meredith, '47). While the suggestion that the increase is due to ''improved standards of nutrition'' is plausible, several pertinent objections may be voiced. First, it is doubtful that the diet actually improved (except in caloric intake) until the rather recent stress on practical nutrition, the increased consumption of fish, vegetables and milk, and the introduction of enriched and fortified staples. Second, if an increase in caloric consumption is alone responsible for the change in stature, one would expect the rich to show little change as compared with the previously less favored classes. Yet Bowles' own data show that the increase is pronounced at higher economic levels, and Meredith's private school data show the same marked trend within the short span of a decade (Meredith and Meredith, '44). Whatever the responsible factors are, they are active at all socio-economic levels.

This increase in stature is associated with earlier puberty. Present-day American children sprout breast buds, pubic and axillary hair, and attain menarche from one to two years earlier than was formerly true. This appears to be the case in England, except that earlier puberty is thought to be accompanied by an earlier cessation of growth, so that these children mature earlier and stop growing earlier (Morant, '48).

Pubertal changes, once entirely restricted to descriptive studies, are now understandable in terms of the actual hormones involved, as a result of recent advances in endocrinology. In fact, since all of the principal maifestations of "puberty" or "adolescence" are referrable to the steroid hormones of gonadal and adrenal origin, it is likely that the term "steroid growth phase" as suggested by Kinsell et al. ('46), will replace the less meaningful terms "adolescence" (becoming adult) and "puberty" (developing pubic hair). While changes characteristic of the steroid growth phase are mediated by the steroid hormones, differences in the rate and pattern of maturation, and degree of sexual dimorphism cannot be attributed entirely to differences in the amount and timing of the steroid hormones alone. Just as the hair follicles of different individuals respond differently to equal amounts of methyl testosterone, so breasts, buttocks, femora, supertrochanteric fat pads, and biceps muscles preserve their own individuality of response. Individuals muscular before the steroid growth phase become disproportionately muscular thereafter, and rotund, poorly muscled children often gain but little muscularity during the steroid phase. Neither body-build nor the degree of sexual dimorphism are yet explainable in terms of endocrine titers alone, and differences in end-organ or "target-organ" response are still matters for geneticists to investigate.

Changes in size, form and proportions slow down late in the steroid growth phase, and further alterations are commonly ignored. Growth is considered at an end at 18 by the military, and at 21 by the common law. In racial studies we commonly include young adults (those 17 and older) with all adults not overtly senile. Actually, growth and developement continue. Peak androgen production in the male is attained after the 21st year (Hamilton, '46), and we would expect metrical changes for some time afterward. Actually, stature (in males) continues to increase until the mid-twenties (Randall, '49), muscularity increases for a proportionate length of time, and some serum constituents, which rise rapidly during the steroid growth phase (Garn and Gertler, '51), continue to increase for many years (Gertler, Garn and Bland, '50).

Since the endocrine determinants of size and build do not reach their maximum until after legal majority, and increases in stature and muscle-mass may continue for some years, environmental modifications (apart from those due to exertion) may occur during the third decade. The recent work of Lasker ('51) demonstrates the extent to which this is possible. Mexicans who have resided in the United States, even temporarily, differ in size and proportions from their less perambulatory neighbors; the differences are a function of length of residence, and age at emigration.

Even after the 4th decade, when increases in gross size and proportions come to a halt, individual organs and tissues may continue to grow. The ears, the nose, and the fingers enlarge under anterior pituitary stimulation, the teeth emerge further, and the epithelium thickens. However, quantitative studies are still lacking. Moreover, it becomes difficult to draw a dividing line between normal growth and pathological overgrowth, changes in the joints, the skin, and the intima. And the growth potential may become a liability; normal growth in some tissue may suddenly change to uncontrolled, invasive neoplastic growth. For this reason, research in growth and development, especially on the cellular level, is closely linked to cancer research.

There is another aspect of growth and development of additional importance beyond the 4th decade, and that is the tempo of growth. Between species, we know that the rate of maturation and the length of life are closely related. Is that true within the human species as well? At present this is an important problem. There is some evidence that coronary artery disease and breast cancer appear earliest in those who mature earliest. And while the female matures earlier than the male, her longer life expectancy may be attributed to her lower oxygen uptake, her smaller rate of energy expenditure. There is, then, some indirect evidence that longevity and the tempo of growth are inversely related, but the direct evidence must come from longitudinal studies. In all probability the longitudinal investigations at the Fels Research Institute, at Harvard, and at Denver will help to illuminate gerontological problems as well as problems in growth and development, thus proving that research in growth and development reaches from fertilization to demise, from sperm to worm.

APPRAISING INDIVIDUAL GROWTH

As shown in the preceding sections, the scope of research in growth and development is wide, extending from the time of conception well past the 4th decade of life. Problems in growth and develpoment are similarly varied, ranging from prenatal nutrition to adult changes in build and size. Quantitative information on growth and development is ordinarily derived from the study of grouped data; such problems as we have mentioned are investigated by the study of large numbers of individuals. But ultimately, information on growth and development must be applied to the evaluation of the individual. This is the bread-and-butter task of growth research. We develop, use, and constantly seek improved techniques to differentiate between normal and pathological growth, between satisfactory growth (as we know it) and unsatisfactory growth.

Nearly all techniques for individual growth appraisal employ measures of height and weight, and the majority use height and weight alone, not because these two variables are sufficient, or even the best criteria of growth, but because they are most commonly recorded. Usually, height and weight are plotted separately in relation to population norms, thus showing relative status, and (with serial data) whether the relative position is maintained. A few techniques also combine height and weight to obtain a measure of ''physique,'' and thus employ changes in height, weight and "physique," as indications of the progress of growth. Less commonly, other measurements and maturational indicators are added, using changes in developmental status as well as changes in size, as measures of growth sufficiency.

The simplest approach to the problem is that of Turner and Nordstrom ('38). In this system, height and weight are measured at regular intervals and children who fail to gain for two successive periods are referred for further study. The primary advantage of this method is that it can be employed without the selection of norms. It is particularly applicable to public-health surveys, and works much like mass chest x-rays, selecting out those individuals who are probable causes for concern.

Most systems of growth appraisal employ norms, and it is therefore important that the right norms be used. They should be recent (not over 10 years old), and they should apply to the individuals in question. Current height-weight charts escape somewhat from the rigidity imposed by norms alone by evaluating growth in terms of percentiles. Attention is directed to the relative size of the child, and whether he retains this relative position, or whether he loses percentile status. In the charts prepared by H. C. Stuart and others, loss in relative position is an indication for further examination.

While the charts of Stuart, Meredith and others have built-in norms, as expressed by the percentile curves, and are thus applicable to specific populations, the Fels Composite Sheet (Sontag and Reynolds, '45) is applicable to any population, and allows the user to select his own norms. The Fels Sheet is otherwise just a variant of the preceding system. Height, weight and other measurable data are plotted in terms of standard scores, and children who drop off from their standard score values are considered as possibly unhealthy.

In the "grid," N. C. Wetzel relates the logarithms of height and weight to give a measure of body-build or "physique," and follows growth in terms of changes in the log-log relationship (Wetzel, '41, '46a, b). This procedure tends to straighten the growth "curve" into a line, since volumetric growth is indeed exponential rather than linear. Growth deviations are thus shown as aberrations from the straight-line progression, and to some extent the Wetzel grid gets away from the problem of the selection of norms for particular groups or individuals.

Other systems, though proposed for general use, are still rather restricted to clinics and longitudinal growth studies. Stuart and Meredith suggest anthropometry as a routine practice for school and office (Stuart and Meredith, '46). Todd ('37) and later, Greulich and Pyle ('50) have brought the techniques of skeletal age determination to a high peak of perfection. Dental age can be employed (Cattell, '26; Hurme, '49). Measures of body fat, either by the Franzen caliper or the x-ray method are practical, and in his work McCloy ('36) used height, weight, the height-weight index, and various measures of tissue components in the evaluation of growth.

All of these techniques, however, do fail in one or more respects, and it is often true that "normal" children are sources of undue concern, while subclinical deficiencies escape the net. Even though we may stress *relative* status rather than absolute size, what of the child who consistently remains at the first percentile? Should this be a reason to worry, or not? Even though a child's weight-height relationship is maintained, what about those extremes who hold relative position, but are still extremes. Depending on our personal prejudices we view children in the Wetzel A_4 and B_4 "channels" as "normal extremes" or alternately, as potentially unhealthy individuals.

What about these extremes? Extreme fat or extreme linearity may be aesthetically unpleasant, but are these extremes more or less pathological than extreme muscularity? Are we right in worrying about the child in the lowest percentile while applauding the correspondingly aberrant child in the highest percentile? During the first year of life pediatricians are delighted by infants who quadruple or quintuple their weight, while at later ages correspondingly disproportionate increases are viewed with apprehension. We admire the lean octogenarian, but attempt to fatten the lean 10 year old. At present we have no principles, only prejudices, to cover these examples.

The same is true of deviations from those growth lines so impressively drawn upon the charts. Actually a child who holds to one of the percentile lines for any prolonged period of time is a rarity. Most children deviate: they go up and down. Now they are at the 75th percentile, and now at the 50th. In the Wetzel grid the same is true. Only a minority of children plod along in the same "channel" year after year (Garn, '50, '51). To what extent should we be concerned by these smaller deviations, minor falling-away from position?

One explanation for statistically abnormal growth behavior comes from differences in maturational rate, which probably represent individual constitutional differences. Rapid-maturing children climb in percentile status: they "overshoot" the trend lines. In contrast, slow-maturing children often appear to lose status, not because they fail to grow, but because the majority are growing faster. They "undershoot" the trend line. The growth curves and charts now in use do not make allowances for such differences in the rate of maturation (see Tanner, '51). However, H. C. Stuart and his associates are now preparing growth curves that will allow for differences in maturational rate and the pattern of growth. (Personal communication.)

As indicated previously, most measures of growth and development (except for Wetzel's) are strikingly unconcerned with the meaning of differences in physique and in developmental status. Some workers, such as Todd ('37) have made the assumption that retarded osseous development necessarily indicates impairment of health.[3] Others have assumed that scanty body fat is pathognomonic, or conversely, if maintained, that it is normal: we do not know which. With extremely short stature, provided that percentile position is not lost, we are unable to say whether it is truly "genetic," or symptomatic of unsatisfactory growth. And in fact, we do not know whether the American emphasis on growth curves, norms, and measures of maturation is generally wise (in the sense that it has value in preventive medicine), or whether the French are wiser in ignoring this mathematical armamentarium under the belief that it does more psychic harm than physical good.

What we do need is detailed information on the long-term growth and development of large numbers of individual children, wherein differences of physique, size, pattern and rate of growth are investigated and evaluated. (We have about exhausted the potentialities of averaged growth curves derived from massed data.) In order to apply our knowledge of growth and development to individuals we must know more about individual growth and development, and the growth and development of individuals similar in certain respects.

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[3] "On the other hand a child retarded in maturity rating twelve months or more on our standards below his chronological age has experienced a setback in constitution from which he has not yet recovered even though he show no signs of ill-health and neither parents nor physician can recall anything in the past history which would lead one to suspect constitutional disturbance." (Todd, '37, p. 22.) But a 12 month "retardation" is by no means excessive and equals only 1 S.D. at 10 years of age. It is therefore extremely doubtful that a 12 month "retardation" has the clinical significance that Todd claimed.

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A Biometrical Rule.— The body-weight/body-surface ratio decreases more or less regularly from north to south, and the lowest averages are found in hot climates. Manifestly the decrease of the weight/surface ratio in human beings is an anthropological — and strictly biometrical — equivalent of the two well-known zoological ''laws'': Bergmann's rule (body-size of related homeotherms decreases in the warmer parts of their geographical range), and Allen's rule (relative increase of protruding organs in warmer districts). I think that a biometrically correct rule may be generalized in the following way: in closely related homeotherms, the body-mass/body-surface ratio tends to decrease in climates which, at least during a part of the year, put a stress on heat-eliminating mechanisms.— Eugène Schreider. Anatomical factors of body-heat regulation. Nature, vol. 167, no. 4255, May 19, 1951, p. 823–824.

APPLIED PHYSICAL ANTHROPOLOGY

ROBERT M. WHITE
Quartermaster Climatic Research Laboratory, Lawrence, Mass. [1]

Scientific research in general consists of two main efforts, not always clear and not necessarily mutually exclusive, but nevertheless usually distinguishable. The first includes investigations which primarily add to our body of knowledge. These are largely the result of our intellectual curiosity. The second comprises studies oriented toward useful or practical applications. These are the result of our desire for improvement of one sort or another.

This situation is particularly true in anthropology — the study of man. Being insatiably curious about ourselves, we study evolution, cultures, linguistics, morphology, genetics, and we classify races, implements, body builds and blood types. We investigate primates, environments, growth processes, and human relations, to name but a few topics at random. We also try to pick the right man for the right job, improve nutrition, correct malocclusion in children, design more efficient machines, and even develop better fitting clothing.

In some scientific disciplines such as mathematics, physics, chemistry, or engineering, careful distinctions are made between pure and applied fields. It does not seem necessary, however, to delimit separate categories of theoretical and practical work in physical anthropology, since most, if not all types of research in physical anthropology have, or at least should have, ultimate applications of some sort.

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[1] The opinions or assertions contained herein are the private ones of the writer as a physical anthropologist and are not to be construed as official or as necessarily reflecting the views of the Quartermaster Corps or the Department of the Army.

The applied concept of course is not new in anthropology. The term "applied anthropology," for example, is used by the Society for Applied Anthropology, which states its primary objective to be "the promotion of scientific investigation of the principles controlling the relations of human beings to one another, and the encouragement of the wide application of these principles to practical problems." Another instance of practical application in which the techniques of anthropometry are utilized is the general field designated as "human engineering." This term is used primarily by experimental psychologists and applied engineers working on biomechanical problems. However, from the viewpoint of a physical anthropologist, human engineering may be defined as that endeavor which attempts to design and build modern machines to fit human beings so that their utilization by man will be comfortable, safe, and more efficient.

The term "applied physical anthropology" may be used conveniently with reference to those studies or problems in physical anthropology in which the practical applications of techniques or results are emphasized. In some respects this is not an entirely satisfactory definition but it is a useful one. The lack of an adequate descriptive title for this area of physical anthropology actually is no great problem if a broad perspective is adopted. The practical application of the techniques and findings of physical anthropology becomes, then, more of a point of view which may receive varying degrees of emphasis depending upon the problem at hand, rather than a separate and specialized field of endeavor.

Potential applications in physical anthropology in a broad sense are many and diverse. Physical anthropology has made many significant contributions in the fields of medicine and dentistry, including growth studies, pediatrics, orthopedics, orthodontics, and prosthetics. Another important field in which some of the techniques of physical anthropology are utilized is in legal medicine, identification (both civilian and military), and dermatoglyphics. The comparatively recent advances in human genetics are of great interest to physical anthropologists and have many potentially important applications. These are some of the more general applications of physical anthropology.

More specific applications of physical anthropology may be found in studies concerning the human body in relation to its physical surroundings. Hooton's study of seating for trains and the anthropometric survey of women and children carried out by the Bureau of Home Economics for the improvement of clothing sizing are good examples. This is the type of work which most people probably think of as being applied physical anthropology.

In 1948 a symposium on applied physical anthropology was held as a part of the program at the annual meeting of the American Association of Physical Anthropologists. Some idea of the scope of applied physical anthropology is indicated by the topics discussed, which consisted of skeletal identification, identification of war dead, use of anthropometry in machine design and in clothing, and military anthropometry in Great Britian, in the Air Forces, and in the Quartermaster Corps.

Our present national emergency again emphasizes the military applications of anthropology. Some of the techniques and findings of anthropology have been utilized in psychological warfare and in the military and civil governments of occupied areas. The military uses of physical anthropology date as far back as the Civil War, and have been on the increase ever since. A large share of the current work in applied physical anthropology is supported by the Armed Forces. Anthropometric surveys of large samples of military personnel have provided the necessary data on body measurements. These data are analyzed statistically and then applied in military research and development problems. During the last war, for example, the Air Force made use of anthropometric data in studies of spatial requirements in aircraft and in developmental work on flight clothing and other personal equipment. This program is continuing.

In the Army, the Quartermaster Corps is utilizing anthropometry primarily in the development of better fitting and more efficient clothing. The basic application here consists of correlating clothing sizes with body sizes in order to fit the largest percentage of the military population with the most economical number of sizes requiring the least amount of alteration.

With respect to technique, physical anthropology, or more particularly anthropometry, as such, has a long and well-established history of convention and orthodoxy. Many of the standard body measurements are useful and much of the work which has been done over the years has been of value. More recently, however, changes have been taking place and we find ourselves questioning and re-examining many of our methods in physical anthropology. The days of measuring just for the sake of measuring and amassing data seem to be over. With regard to applied physical anthropology, flexibility and adaptability are essential, so that conventional techniques may be modified or expanded to meet the new and changing requirements of practical problems. Anthropometry, in addition to the standard measurements of the static man, may now include dynamic and functional measurements, so that its utility is becoming considerably broadened. Students in physical anthropology, in addition to a thorough background in classical methods and techniques, should be introduced and exposed to the applied point of view. More interest in the expanding possibilities of applied physical anthropology should be developed and encouraged. The literature on applied physical anthropology is scattered but the number of available papers and articles is sufficient to indicate the types of problems which may be investigated.

In a sense, work in applied physical anthropology, in addition to being an opportunity, is also a responsibility. The physical anthropologist should not merely collect and analyze the necessary anthropometric data; he has the additional responsibility of interpreting the results and applying them to the problem at hand. Also, the physical anthropologist in this field is often called upon to collaborate with other specialists in a joint effort. He may find himself working with an engineer, psychologist, environmental physiologist, industrial designer, orthodontist, or bio-physicist, to name but a few possibilities. The position of the physical anthropologist is unique in that he is equipped to contribute the data, as well as the point of view necessary for the successful integration of human and material elements. In other words, it is up to the physical anthropologist to get more of the man into the picture.

Workers in applied physical anthropology need not confine themselves only to immediate problems. Thus the data utilized in the solution of practical problems should also be explored for possible contributions toward theoretical problems. Some of the methods and techniques developed during the course of practical investigations may have wider utilization. In this way, applied physical anthropology can fulfill a useful and profitable function in the field of anthropology as a whole.

Applied physical anthropology, then, is an approach in which attention is focussed upon the solution of practical problems. It is varied in scope and often utilizes modifications or adaptations of conventional techniques. It is a point of view which permits expansion in the general field of anthropology, as well as collaboration with workers in other fields of endeavor.

With regard to the literature, there are no standard textbooks on the subject of applied physical anthropology; nor are there any references which cover adequately all of the possible topics or problems in this diversified field. Since a large part of the work usually considered to be applied physical anthropology has been in military research and development, many publications have been in the form of official military reports. Some of these carry a military security classification; in general, the military reports are somewhat difficult to obtain.

The following annotated bibliography is not a comprehensive "reading list," but rather is suggested as a group of references to indicate the scope and nature of some types of applied physical anthropology.

Krogman, W. M. 1951 The role of physical anthropology in dental and medical research. Am. J. Phys. Anthrop., n.s., *9* (2): 211–218.

This is a recent and useful summary, in outline form, of the problems and studies in medicine and dentistry in which applied physical anthropology has been utilized.

O'Brien, R., and M. A. Girshick 1939 Children's body measurements for sizing garments and patterns. A proposed standard system based on height and girth of hips. U. S. Dept. of Agr., Misc. publ. no. 365. U. S. Government Printing Office, Washington, D. C. 25 pp.

O'Brien, R. A., M. A. Girshick and E. P. Hunt 1941 Body measurements of American boys and girls for garment and pattern construction. A comprehensive report of measuring procedures and statistical analysis of data on 147,000 American children. U. S. Dept. of Agr., Misc. publ. no. 366. U. S. Government Printing Office, Washington, D. C. 141 pp.

O'Brien, R., and W. C. Shelton 1941 Women's measurements for garment and pattern construction. U. S. Dept. of Agr., Misc. publ. no. 454. U. S. Government Printing Office, Washington, D. C. 73 pp.

The three references above report on a Works Progress Administration project carried out between 1937 and 1940, and sponsored and supervised by the Bureau of Home Economics. While many large series of children have been measured, most studies of body measurements have been restricted largely to growth and other anthropological problems which have not included the measurements used for garment sizes. As the first study of body measurements and clothing for women and children, this project represents an interesting application of physical anthropology.

Symposium on Applied Physical Anthropology 1948 Am. J. Phys. Anthrop., n.s., *6* (3): 313–380.

T. D. Stewart. Medico-legal aspects of the skeleton: Age, sex, race and stature.

Charles E. Snow. The identification of the unknown war dead.

G. M. Morant. Applied physical anthropology in Great Britain in recent years.

Barry G. King. Measurements of man for making machinery.

Mansfield Lonie. Anthropometry and apparel.

H. T. E. Hertzberg. Post war anthropometry in the Air Force.

F. E. Randall. Anthropometry in the Quartermaster Corps.

The titles of these papers are indicative of some of the applications of physical anthropology, particularly in military work. Most of the papers include a list of additional references on the subjects concerned.

Morant, G. M. 1947 Anthropometric problems in the Royal Air Force. Brit. Med. Bull., *5* (1): 25–31.

Darcus, H. D., and A. G. M. Weddell 1947 Some anatomical and physiological principles concerned in the design of seats for naval war-weapons. Brit. Med. Bull., *5* (1): 31–37.

Both of these articles were reprinted in the *Yearbook of Physical Anthropology, 1947.* Some interesting military applications of anthropometry in Great Britian during the last war are discussed.

Randall, F. E., A. Damon, R. S. Benton and D. I. Patt 1946 Human body size in military aircraft and personal equipment. U. S. Army Air Forces Technical Report no. 5501, June 10. Air Materiel Command, Wright Field, Dayton, Ohio. 333 pp. (Unclassified.)

This volume is a compendium of material resulting from the work in applied anthropometry carried out during the last war at the Aero Medical Laboratory. It is a useful reference since it represents a synthesis of a large number of Air Force reports. Discussions of theory and methods are included, together with anthropometric data on Air Force personnel.

Randall, F. E. 1948 Applications of anthropology to the determination of size in clothing. Environmental Protection Series, Report no. 133, June 30. Quartermaster Climatic Research Laboratory, Lawrence, Mass. 108 pp. (Unclassified.)

This report will serve as a useful introduction to the applications of anthropometry and statistics in military clothing research.

——— 1949 Anthropometric nomograph of Army men. Human Biology, *21* (4): 218–232.

This material was originally published in January, 1949 as Environmental Protection Section Report No. 147, but the article in *Human Biology* is more readily available. The nomograph represents a method of reducing a large mass of anthropometric data to a convenient and simple form.

Tufts College Institute for Applied Experimental Psychology (Handbook Staff of) 1949 Handbook of human enginering data for design engineers. Technical Report no. SDC 199-1-1, Nav Exos P-643. (Contract N60nr-199, T. O. 1), December 1. Special Devices Center, Office of Naval Research. (Unclassified.)

As indicated by the title, this is a reference volume. Subjects covered include experimental techniques and statistics, dimensions of the human body, vision, hearing, skin sensitivity and proprioception, motor response, physiological conditions, and intelligence. The anthropometric data are somewhat limited.

Abt, L. E., and others 1951 Human Engineering. Ann. New York Acad. Sci., *51* (7): 1123–1278.

This series of papers is the result of a conference on human engineering, held by the Section of Psychology of the New York Academy of Sciences on May 10 and 11, 1948. Included are two articles on human locomotion and three articles on prosthetic devices which illustrate some of the functional or dynamic applications of anthropometry.

ANCIENT MAN IN AFRICA.— Northern Rhodesia has so far yielded only a few human relics which can confidently be assigned to any part of the Stone Age. Among these, the famous skull and other fragments found at Broken Hill in 1921 take pride of place. The Italian Scientific Epedition of 1930 was responsible for several valuable finds, which were entrusted for description to Prof. R. A. Dart of the University of the Witwatersrand. More recently Dr. Desmond Clark has several times secured human remains more or less definitely associated with Stone Age industries, and all these have also been sent to the University of the Witwatersrand for study. . . .

[The data obtained from examination of these specimens] are at least sufficient to indicate that the pattern of human types in Northern Rhodesia does not coincide with that of either South Africa or East Africa, but appears to combine features of both. It would be easy, but surely premature, to elaborate hypothetical explanations of these regional divergences. For the time being we may accept them as demonstrating how crucial a position Northern Rhodesia occupies for any understanding of the history of human types in Africa.— L. H. Wells. Fossil man in Northern Rhodesia. Appendix C (pp. 143–150) in J. Desmond Clark. The Stone Age cultures of Northern Rhodesia. With particular reference to the cultural and climatic succession in the Upper Zambezi Valley and its tributaries. South African Archaeol. Soc. Monograph no. 1, 1950, x + 157 pp.

AIR FORCE ANTHROPOLOGY IN 1950

H. T. E. HERTZBERG AND GILBERT S. DANIELS[1]
Anthropology Unit, Biophysics Branch, Aero Medical Laboratory, Wright Air Development Center, Wright-Patterson Air Force Base, Dayton, Ohio

TWO FIGURES

This paper[2] reports certain activities of the Anthropology Unit of the Aero Medical Laboratory at Wright-Patterson Air Force Base in 1950. It has been stated previously in this Journal, but can bear repeating, that the Unit is responsible to the Air Force for information on: first, human dimensions; secondly, human muscle strength; and thirdly, human comfort (insofar as this last is dependent on the dimension of workspace or personal equipment). We shall describe briefly a representative example of the sort of work we did in each of these categories during the past year.

In the dimensional field, our major preoccupation was the Anthropometric Survey. This is not yet complete, as we have only reached the stage of statistical reduction; but it may be worthwhile to outline some of the unorthodox methods by which we met the demands for unusual information.

Anthropometry started with the measuring of skeletal, and later of bodily, diameters. But clothing and personal equipment do not fit diameters. The development of new types of equipment conforming closely to body surfaces has demanded extensive information on how a man's skin fits him. Because this information did not exist, we decided on a new survey.

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[1] Second Lieutenant, USAF (MSC).

[2] Presented at the Twentieth Annual Meeting of the American Association of Physical Anthropologists, March, 1951, at the University of Michigan, Ann Arbor.

We let a contract to Antioch College, Yellow Springs, Ohio, for the services of 12 students to form a measuring team and for statistical reduction of the metrical data to be gathered. After intensive training in necessary anthropometric techniques, these men made a tour of some 16 Air Force Bases under the constant supervision of one or the other of the authors. The team took 132 measurements and observations, and some sociological data on a series of about 4,050 Air Force flying personnel, plus 4 somatotype photographs of each.

In planning to use our team in the most efficient manner possible, we listed all the measurements needed, old and new, and analyzed them as a time-motion study. The 12 men were grouped into 6 pairs, the measurer and the recorder interchanging at intervals to relieve the strain. Each pair had its own station and tools in the "production line," and measurements were so allotted as to eliminate the making and breaking of the anthropometer, and to minimize the picking up and laying down of instruments, the changing of subject positions, and other wasteful motions. The sequence of measurements in general was from the top downward and outward in logical progression for any body member. The blank was arranged to simplify the recording for each group. This system worked so well that, when enough subjects were available to keep us busy, we could measure 140–150 in 7 hours. A man could easily come in, go through the line, and emerge within about 15 minutes, and sometimes less. He generally remarked, in a rather dazed way, that he'd never had so thorough and purposeful a going-over in his life.

Most of our measurements are conventional or taken from the Quartermaster handbooks written by Dr. Randall ('47, '48); but some 35 are new, devised for specific Air Force needs. A fault of previous surveys from a functional standpoint has been the measurement of the body in fixed and unnatural postures, without consideration for its ability to move and to stretch. Therefore we introduced a number of measurements intended to indicate the space "envelope" within which the man performs his tasks. One essential dimension is interscye maximum. We measured interscye between the scye marks as shown in the Quartermaster handbook, and then had the subject throw his shoulders forward to produce the maximum interscye distance. This body stretch is an important factor in tightly-fitting garments. Figure 1 illustrates a few dimensions taken on the head for various purposes. These measurements are not entirely satisfactory because they do not completely define a surface; but they do provide

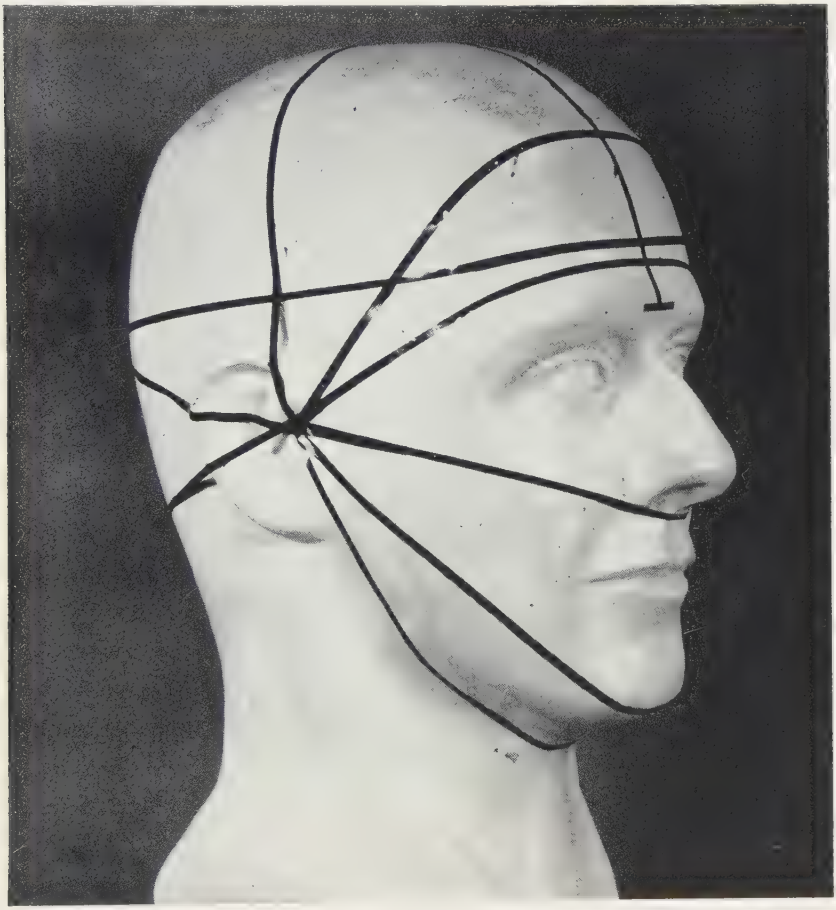

Fig. 1 Anthropometric measurements of the head.

closer approximations to the body surface and volume than were available heretofore. We have tried, moreover, to tie together as many as possible of our measurements so as to delineate head and body volume without ''floating'' zones; i.e., zones whose positions in space are unknown because they have not been related by measurement to known points. We have not fully succeeded, but work is continuing toward the ideal of a complete metrical specification of the body surface. Once the human topography with its variability is statistically known, a reasonable approximation of body size and shape for any application will become possible as a starting point for design. Certainly the ingenuity of designers seems limitless in devising dimensional demands we cannot satisfy without special research.

Following the requirement for speed with accuracy in our survey, we adapted the measuring-board technique to a number of uses. We made up grid-sheets in centimeters and millimeters and tacked them to the wall in a corner of our measuring room. The distance from the corner of the room was carefully scaled by tape or anthropometer. In that way, we measured arm length, maximum reach, and functional reach by placing the subject's shoulders against one wall of the corner, and using a square block at the fingertips. Foot length, foot breadth, and heel breadth were similarly measured on another special board.

In some cases, we found it saved time to fabricate our own instruments. In one instance, on the foot measuring board, we took the heights of the lateral and medial malleoli by means of a block with a millimeter scale pasted along its vertical edge. In another case, we wanted to determine the maximum diameter of the largest finger. Rather than waste time with a clumsy and relatively inaccurate caliper operation — because of flesh softness — we set up a plastic sheet with holes of carefully graded sizes drilled in it (fig. 2). When the subject had found a comfortable fit, we had an accurate and realistic measure of the diameter. In these ways, we economized not only on subject-time, but on instruments as well.

Parenthetically, it might be mentioned that, although we use the metric system for measurements, we must translate our figures into the English system for use by engineers, tailors, and others. One tool we were able to have made on special order was a $2\frac{1}{2}$ m steel tape with a metric scale on one side, and inches and tenths of inches on the other — a unique and very useful combination.

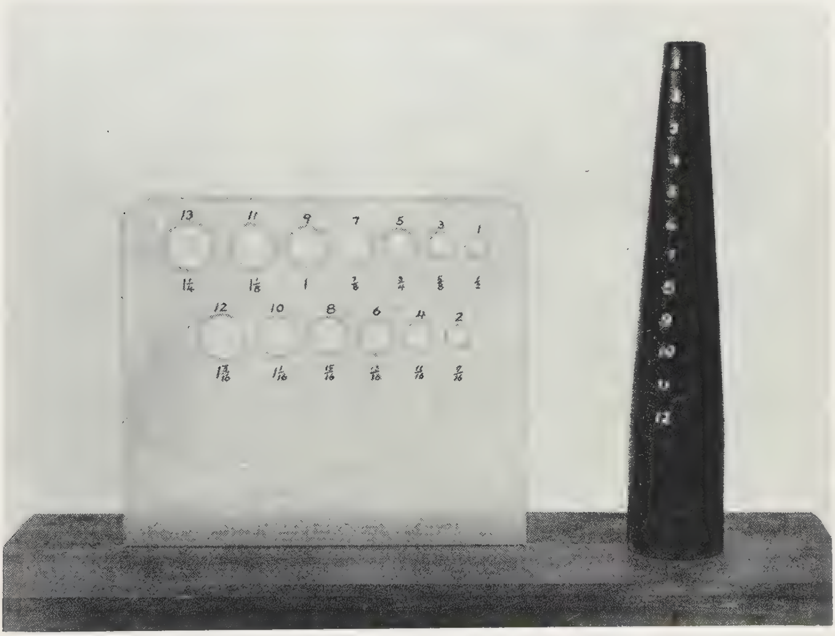

Fig. 2 Finger Size Measuring Board. (To the right is shown the Grip Diameter Measuring Stick.)

The second aspect of our responsibility is exemplified by the foot-pedal angle-and-torque study. This was an attempt to measure not only pilot muscle strength in the actuation of cockpit brake pedals, but also the angle at which the muscle force can be most efficiently applied. The subject sat in a seat accurately simulating actual body position in a cockpit. Then he exerted maximum pressures on the rudder pedal with his foot flexing forward about his ankle. Forces on the pedal were measured by strain gauges mounted on a stress ring. Measurements were taken for both the neutral and the extended pedal positions, and at various pedal angles in each. For both positions, the pedal angle of optimum efficiency was found to be about 25° forward of vertical. This pedal angle is likewise the most comfortable for the foot and leg in the several circumstances.

The provision of comfort, our third responsibility, entails a combination of both dimensional and muscular data, and something else. It is a fascinating art that so far is entirely subjective. At present, we can mention only a few examples: the prone position bed for pilots, the supine seat for pilots, and the control stick grip development for fighter airplanes. The first has been briefly described previously in this Journal (fully in government publications); the second is unpublished as yet; the last is an effort to provide the pilot with a comfortable and functional stick grip for combat use. To achieve a scientific basis for this problem, we had to design an entirely new instrument for the measurement of the inside of the gripping hand. The device and the stick grip partly based on these palm measurements are illustrated in another paper by the same authors.

There is another fundamental aspect of the body which touches our work. For example, it has been necessary heretofore to determine the center of gravity of a man for each specific condition of body position and loading with equipment. Such information is frequently needed by airplane designers for calculations of stresses in ejection seats, and other specialized purposes. We developed a method of calculating the center of gravity of an average man for any body position or condition of equipment load, provided the load was known. It employs the method of moment forces with reasonable approximations to the weights and centers of gravity of body segments.

Our plans for the future include a considerable expansion, because of the greatly increasing utility of Anthropology Unit activities in the Air Force mission. Particularly do we need fundamental research on the body as a mechanical structure. Existing data on center of gravity and weight of body segments, for example, were collected in Germany over 70 years ago on two series of only two and three cadavers, each of "sturdy build." Such work should be repeated on an adequate series of known somatotype by clear techniques. A study should be made of the position of limb axes with respect to body surfaces in different body positions. We hope also to make a very complete study of the entire body in the fashion described for the foot pedals. And the list can be considerably extended.

Thus far, this paper has catalogued some of the applied physical anthropology done for the Air Force in 1950–51, and indicated some of our hopes for the future. But we want it to do something more. Our work and our plans for expansion of this sort of service spotlight a major anthropological opportunity. Our work can be an outlet for those young men who reluctantly turn away from physical anthropology because of the present limited opportunity. Applied anthropometry is useful, not only for military needs, but also for society at large.

For some years, the idea has been current that only a mechanically-inclined anthropologist could hope to do applied anthropometry. Although this idea probably was true at one time, it is so no longer. The field of applied physical anthropology is so new and so ramified, as the foregoing account of our activities and plans indicates, that it now needs the assistance of physical anthropologists of all interests. The range of problems is so large that the Air Force Anthropology Unit, at present (March, 1951) manned by only two anthropologists, could expand considerably. Other branches of the military and naval services, not to mention civilian aeronautical agencies, could potentially absorb comparable numbers. Does the young anthropologist like anatomical studies? We need to have fundamental inquiries made into the location of limb axes with respect to body surface; we want studies of body volumes, centers of gravity and weights of body segments. Does he prefer statistical pursuits? We have years of such work ahead. Does he wish to specialize in somatotyping? We have in being a program to conduct longitudinal studies on a large series of Air Force flyers. All this and more, in addition to the gadgetry that popularly characterizes our activity. Thus we have opportunities to satisfy a large variety of scientific tastes — but we lack men. Incidentally, Anthropology Unit material can be used for a doctoral dissertation by the man who does the work.

Many problems of modern warfare — high-velocity flight, for example — now are best solved by team effort. Physiologists, psychologists, and anthropologists collaborate to advise the design engineer. If the anthropologist be absent from that team, the work of bending metal will not stop; the aircraft will get built anyway; but an opportunity, and what has come to be a respected and valuable point of view, will have been lost. And it is reasonably certain that the crew men will be less comfortable.

Finally, then, this paper is intended as a plea, first to young men to consider the field seriously as a life work; and secondly to department heads to recognize the field and its opportunities for men, and to grant it its proper importance as a promising outgrowth of academic physical anthropology.

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APPLIED ANTHROPOMETRY OF THE HAND[1]

GILBERT S. DANIELS[2] AND H. T. E. HERTZBERG
*Anthropology Unit, Biophysics Branch, Aero Medical Laboratory,
Wright Air Development Center, Wright-Patterson Air Force Base,
Dayton, Ohio*

FOUR FIGURES

With the advent of high speed aircraft, the need for greater centralization of aircraft control has gradually resulted in the placing of many of the control functions of the fighter airplane directly on the stick grip. Figure 1 shows the increasing complexity from the simple B-1 grip of 1928 to the presently used B-8 grip. The number of switches that are best located on the grip for operational reasons has made it increasingly difficult to design a grip that is slim enough for comfort and efficiency in gripping, and at the same time, large enough to accommodate the switches and wires necessary for the control functions. An additional problem is encountered in locating the switches so that they may be actuated by the fingers without undue difficulty of movement of the hand on the grip.

Previous grips of this type have been designed by engineers with prime consideration being given to the mechanical nature of the problem. The structural strength and reliability of the switches have been the usual limits of thought in a new design. On occasions when the location of switches became a really critical problem, some consideration was given to their placement, but the end result, in many cases, has been a grip that fitted best only the designer's own hand. The problems of size and comfort, and indeed, operational ease, have generally been considered as secondary design features. We have attempted in the present study to reverse this design procedure by considering first the man who is to use the stick grip. In addition, we have attempted to produce information directly usable by the engineer, and to this end the English system of measurements has been used for all items directly relating to design.

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[1] Presented at the Twentieth Annual Meeting of the American Association of Physical Anthropologists, March, 1951, at the University of Michigan, Ann Arbor.

[2] 2nd Lt., USAF (MSC).

All the subjects in this study were trained Air Force pilots, and the anthropometric data collected on this series corresponded well to the larger sampling of anthropometric data gathered in the 1950 Air Force Anthropometric Survey (described in another paper by the same authors). The hand study group had an average age of 28.9 years compared to 28.8 years for the pilot group of the Anthropometric Survey subjects. Heights and weights were both slightly higher for the smaller group — 177.5 cm and 171.7 lbs. corresponding to 176.4 cm and 165.7 lbs. for the larger pilot sample. In the Survey these heights and weights were actually measured, while the hand study subjects merely reported them verbally. Allowing for the error likely to be involved in this estimation of heights and weights by the subjects themselves, the differences between the two groups were not considered significant, and therefore results of this study are considered applicable to the Air Force pilot population in general.

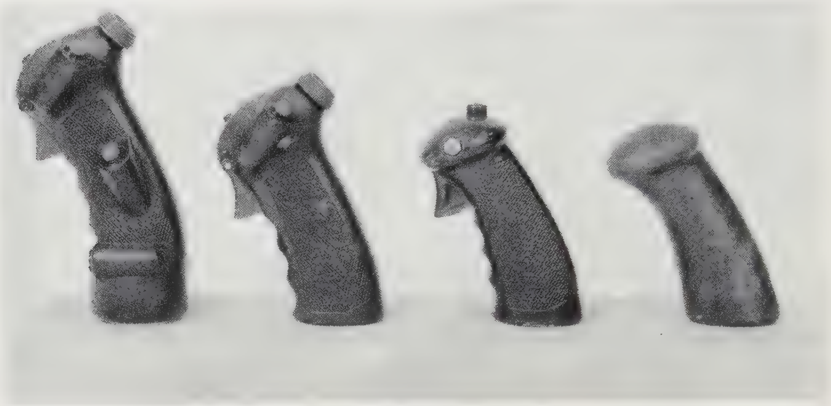

Fig. 1 Development of the stick grip (right to left).

It is usually conceded that the neutral position of a control mechanism is best located so that the controlling limb of the user is at rest in that position. Thus, a control involving wrist motion should have as neutral position one in which the wrist is not flexed. With this in mind, we conducted a primary study to determine, first, a "neutral" position for the mid-sagittal plane of the grip in relation to the centerline of the aircraft at which the combination gripping right hand and stick grip would lie so that the wrist would be in a neutral or rest position. Concurrently with this, we gathered data on range of rotation of the hand on the wrist in the cockpit situation.

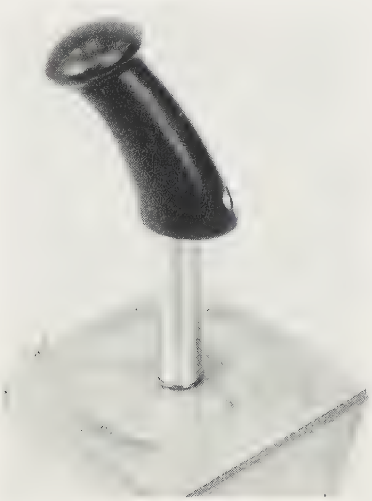

Fig. 2 Test apparatus for determining wrist rotation.

Data were gathered by means of a simplified movable stick grip mounted on a vertical shaft in a simulated cockpit situation. A pointer on the base of the shaft indicated the angulation of the stick grip, with 0° on this wrist rotation measuring device indicating the centerline of the aircraft (see fig. 2).

Each subject was asked to determine the resting position that was most comfortable for himself by grasping the grip as he normally would do in the airplane, and the average of three tries was recorded. He then rotated the stick grip to the

TABLE 1

Rotation of the hand on the wrist

	MEAN [6]	STANDARD DEVIATION	COEFFICIENT OF VARIATION	RANGE OF THE GROUP
Rest or neutral position [1]	19.18° L ± 1.12°	9.95° ± .79°	51.89 ± 4.13	5.3° R to 43° L
Range of motion with control [2]	76.62° ± 2.62°	23.26° ± 1.85°	29.21 ± 2.32	31° to 150°
Control limit left of neutral [3]	45.95° ± 1.76°	15.66° ± 1.25°	34.09 ± 2.71	17° to 86°
Control limit right of neutral	33.61° ± 1.54°	13.66° ± 1.09°	40.66 ± 3.23	6.7° to 67.3°
Extreme range of motion [4]	164.24° ± 2.78°	22.59° ± 1.97°	13.75 ± 1.20	116° to 219°
Extreme limit left of neutral [5]	90.99° ± 2.05°	16.61° ± 1.45°	18.26 ± 1.59	59° to 128.7°
Extreme limit right of neutral	71.82° ± 1.97°	16.03° ± 1.40°	22.32 ± 1.94	44.7° to 119.3°
Control limit left of 0° on measuring device	64.77° L			
Control limit right of 0° on measuring device	14.84° R			
Extreme limit left of 0° on measuring device	110.02° L			
Extreme limit right of 0° on measuring device	54.09° R			

[1] Rest or neutral position: The most comfortable position to the left (L) or to the right (R) of 0° on the measuring device shown in figure 2 or the centerline of the aircraft. The average neutral position of three tries was taken.

[2] Range of motion with control: Total arc in degrees through which subject could *comfortably* move his wrist while still maintaining a usable grip for control of the aircraft.

[3] Control limit left of neutral: } Limit of comfortable and usable control to the left and right respectively of *the individual's*
Control limit right of neutral: } *own chosen resting position.*

[4] Extreme range of motion: Number of degrees through which subject could *possibly* move his wrist without losing his hold on the grip.

[5] Extreme limit left of neutral: } Limit of subject's possible wrist motion to the left and right respectively of *the individual's*
Extreme limit right of neutral: } *own chosen resting position.*

[6] The series numbered 79 individuals, except in all cases labeled "Extreme" where N equalled 66.

left and to the right until he felt that the movement was no longer within the bounds of comfort. Finally, he was asked to repeat this procedure, rotating as far to the left and to the right as he could possibly go without losing his hold on the grip. The results of these measurements are summarized in table 1.

We then carried out a second study to measure the configuration of the heel of the hand and the thumb in the most comfortable gripping position. To accomplish this, a special device was designed and constructed in the laboratory. This is shown in figure 3.

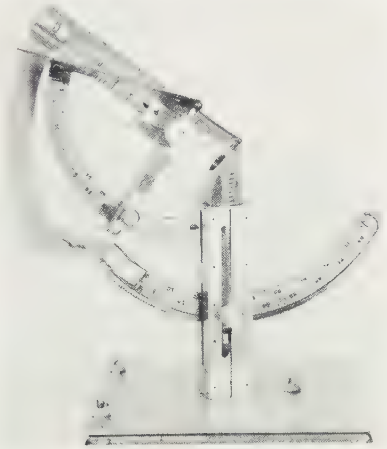

Fig. 3 Device for measuring hand grip contour.

By means of this device, the length of each of the segments of the heel of the hand and the thumb, and the angles between contiguous segments, could be measured. The apparatus was set up in the cockpit mock-up with its mid-sagittal plane turned 19° to the left, as determined for the neutral position in the first study. This angulation remained fixed for the rest of the study. The operator then adjusted the apparatus to a configuration that was considered most comfortable by each subject. The starting point for this was the rest for the heel of the hand. From there each segment of the apparatus was adjusted in turn to the segments of the individual's hand. The average of three comfort adjustments was recorded for the bare right hand, and a single reading was taken for the gloved hand. These data are shown in table 2.

TABLE 2

Configuration of the hand in gripping

	BARE HAND (N = 31)		GLOVED HAND (N = 30)	
	Mean	Range	Mean	Range
Heel of hand width (inches)	1.4	1.1– 1.6	1.3	.8– 1.6
Palm-metacarpal I angle	56.6°	38° –71.3°	60.3°	47° –75°
Metacarpal I length (inches)	1.8	1.1– 2.1	1.8	1.0– 2.4
Metacarpal I — first phalanx I angle	58.4°	45° –73.3°	59.1°	40° –75°
First phalanx I length (inches)	.7	.3– 1.1	...	...
Thumb length (inches)	2.1	1.6– 2.5	...	...

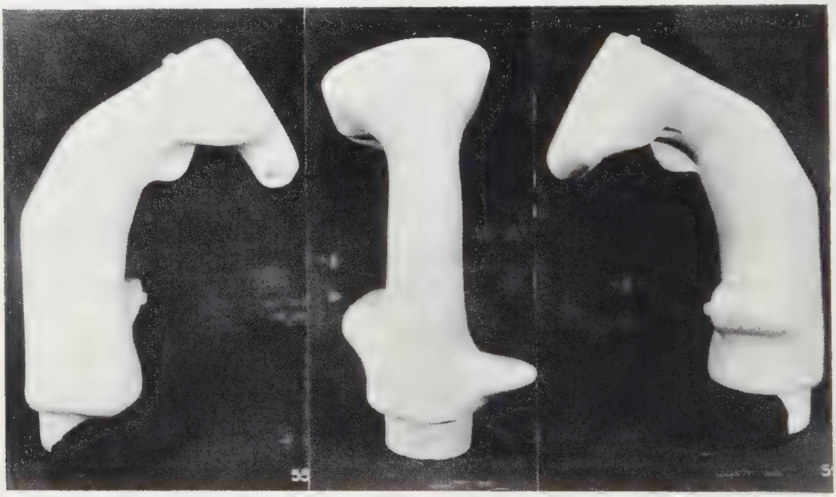

Fig. 4 Proposed stick grip design (three views).

With these data in hand, we have proposed a stick grip design (see fig. 4). The data collected could only dictate one portion of the final design. The remainder of the design work required that each step be checked by actual trial by individuals.

Since, to date, no adequate anthropometric techniques for measuring complex body contours have been developed, this trial and error method is necessary. This principle is recognized by all workers who have attempted to fit a product to the human body. The final evaluation of the grip will be determined by submitting a working model of the proposed grip to a series of pilots for comment.

* * *

Skeletal Collections in Paris.— After the transfer of the Laboratoire d'Ethnologie to the Musée de l'Homme in 1938, skulls and skeletons were hurriedly placed in the storage rooms of the department of Anthropology. The installation of public rooms of this department required all of the staff's efforts. When this installation had been completed, the war hindered the arrangement of the collections. In December, 1941, they still had received only a haphazard classification and still needed to be completely revised. Many of the specimens had deteriorated during their long stay in the gallery of anthropology of the Museum, and others during shipping. Still others had lost their numbers. . . .

Beginning in February, 1942, a systematic arrangement was undertaken. All of the specimens were reviewed one by one. Those which were defective were restored. The identifications were checked and corrected where necessary. Bones and casts were placed in cases in an arrangement based on both geographical and historical provenience; markings on each cabinet permit each specimen to be returned to its place readily. A double catalogue of entries has been set up.

This project, which necessitated a complete review of the archives of the Laboratoire, has taken two and one-half years. . . .

The total collection . . . consists of 12,203 crania and 627 skeletons, with a considerable number (several thousands) of single bones.

The collections of the department of anthropology contain also almost 2500 casts, together with mummies, brains and other wet specimens, a very important series of hair, and a large comparative anatomy series. . . .

This very nice collection is not, however, the only one of its kind in Paris. There is another, composed of the series from the Laboratoire d'Anthropologie de l'Ecole pratique des Hautes Etudes and that from the Société d'Anthropologie de Paris. United under the name "Musée Broca," they comprise approximately 6500 skulls and 230 skeletons, with a large number of casts, wet specimens and comparative osteology series. Although not as extensive as that of the Musée de l'Homme, they nevertheless form a very important collection and one which contains an equally large number of rare and valuable specimens. Now in the hands of the Faculté de Médecine, this collection will be sent to the Musée de l'Homme. When the obstacle embodied in the lack of storage space has been removed, and the fusion of the collections accomplished, the combined osteological collection will be truly the foremost in the world. No museum or laboratory, in Europe or elsewhere, will be able to offer to students of the skeletons of human races resources comparable to those which will be found at the Palais de Chaillot.— Henri V. Vallois. Inventaire des collections ostéologiques du Département d'Anthropologie du Musée de l'Homme. Bull. Mus. Nat. d'Hist. Nat., 2^{e} ser., tome 22, no. 5, Oct. 1950, pp. 543–548.

FAMILIAL HIP DISLOCATION IN OJIBWAY INDIANS.— In a population of 1,253 Indians at Island Lake, in Manitoba, a diagnosis of congenital dislocation of the hip has been made in 45 individuals, ranging in age from 2 to 74 years. There is evidence of other cases that have not been examined, that would bring the total to 71 living persons, representing an incidence of 6%.

The ratio of unilateral to bilateral dislocations was found to be 6 : 5 in the 45 cases. The proportion of males to females was 1 : 6.5.

A genealogical diagram shows a familial character in the incidence of congenital dislocation in this community. All those who were diagnosed, or suspected of having this disease because of reports of anomalous gait were included, whether presently living or deceased. Thus there are 94 cases, or suspected cases, in the chart.

The chart shows that every one of the known or suspected cases of congenital dislocation of the hip at this reserve is connected by blood relationship.— Cameron Corrigan and Sydney Segal. The incidence of congenital dislocation of the hip at Island Lake, Manitoba. Canad. Med. Assn. J., vol. 62, June, 1950, pp. 535–540.

BRIEF COMMUNICATIONS

ARE CERTAIN BLOOD DYSCRASIAS AN EFFECT OF RACIAL MIXTURE?

RICHARD ASHMAN

Heart Station, The Charity Hospital of Louisiana, New Orleans (and formerly of the Louisiana State University School of Medicine)

From the standpoints both of anthropology and of population genetics, the two blood dyscrasias, sickle cell anemia and thalassemia or "Mediterranean anemia," have presented interesting and baffling problems (Neel, '50; Banton, '51). This paper will concern itself with the first of these diseases, though the conclusions may ultimately be found to hold for both. It is not here proposed to consider the clinical aspects of the disease. In America, sickle cell anemia is limited almost exclusively to the Negro. Approximately 9% of the Negro population (Neel, '50) manifests the trait. This is recognized by the appearance on the slide of certain abnormal, often "sickle-shaped" erythrocytes, when the blood is appropriately treated. This phenomenon is called sickling; the condition, which is compatible with good health, is called sicklemia. In other, much rarer, individuals an anemia is present. This is the disease. It appears from clinical studies that the differentiation of trait and disease is not difficult, providing other causes of anemia are ruled out. However, there appears to be considerable variation in expressivity, though penetrance seemingly is practically 100%. Neel estimates that the fertility of persons with the disease may be estimated at something less than 20% of normal. Raper ('50) and also Neel ('50) give data on the incidence of the trait in Africa, and references to the original literature. The incidence ranges from zero to 8.5% in three geographically scattered series of South African Bantus and from 3.5 to 18.9% in Northern Rhodesia. In Uganda, the range in 4 tribes which, though outwardly predominantly Negro, spoke Hamitic languages, was from 0.8 to 3.9%[1]; in 7 different Nilotic language groups, 21 to 28%; in Bantu speaking tribes, 2.0 to 45%, the high value being found in "pygmoids," the average of the others being 19.0%. According to Lehmann and Milne, quoted from Raper, 40% of Mpwapwa adolescents showed the trait. In Angola, two samples were 27% and 8%, the latter being males. The incidence in 5,500 Africans from the Gold Coast, French Togo, Gambia, Sierra Leone, and Nigeria was 12.4%; other samples from this part of Africa range from 16.6 to 28.3%. In the Belgian Congo van den Berghe and Janssen ('50) report incidences of 20.0% among Bantus and 25.9% in Efé Pygmies. Raper estimates that 40 million Africans have the trait. As in North America, sicklemia is found among Negroes in Central and South America.

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[1] But 17.9% in the Teso.

The mode of inheritance of sickle cell anemia has been studied by a number of authors. The family data, which are given by Neel ('50, '51) and Beet ('49), need not be recapitulated here. They are compatible with the interpretation that individuals homozygous in respect to a certain gene have the anemia, whereas the heterozygotes manifest the sickling trait, but not the disease.

The nature of the genetic problem. From the standpoint of genetic analysis, the mode of inheritance proposed by Neel ('49, '50) appears to be satisfactory so far as the situation in America is concerned. The problem is to understand how such a deleterious gene could become established in a population. To account for this, Neel ('50) has carefully considered several mechanisms. These are (a) a possible selective advantage of the heterozygote; (b) a high mutation rate. One might also suppose (c) that through the operation of genetic drift, the gene had become established in a small population and later spread as a result of great expansion of this population and gene flow throughout equatorial Africa.

(a) The way in which a selective advantage of the heterozygote operates is through a heightened viability and reproductive potential. With respect to sicklemia, Evans (quoted by Neel, '50) found an incidence of the sickling trait of 15.5% among 302 native West African soldiers classified as physically fit; "among 259 suffering from acute or chronic disease" the incidence of the trait was 25%. A selective disadvantage of the sicklemic individuals is, therefore, indicated, but it is inferential. Neel ('50) has compared the incidence of the trait in different age groups, and finds, rather consistently in data from both Africa and America, a lower incidence in the older groups. This is again not quite conclusive as evidence for a selective disadvantage, as Neel points out, since it is possible that the sickling trait tends to disappear in older persons, or that they are not represented in the hospital data because of better health.

It has also been suggested that mothers who lose anemic children may compensate by having more. If we suppose that anemic individuals rather rarely reproduce, one may estimate that compensation, in *trait* × *trait* matings, could be brought about only if each mother produced at least three more children on the average than she otherwise would. In view of the low life expectancy under African conditions, this suggestion is improbable. The possibility that *trait* × *normal* matings produce more than the average number of children has apparently not been investigated. The weight of the evidence at this time appears to be strongly against a selective advantage of the heterozygote.

(b) The second proposal advanced to account for the establishment of a harmful gene in a population is a high mutation rate. As Neel points out, one must assume that in parts of Africa, the mutation rate is about 1×10^{-2}. Such a rate, while theoretically possible, is improbably high. Neel and Falls ('51) present a table summarizing the estimates of mutation rates of human genes responsible for various abnormal conditions. The mean rate is estimated to be 2.6×10^{-5}, the range being from 8×10^{-5} to 1×10^{-5}. In this paper, the authors estimate that the mutation rate necessary to maintain the genes for thalassemia or sickle cell anemia is approximately $4\text{–}10 \times 10^{-4}$, but this is evidently based upon the American incidence of the trait, and not on the higher African frequencies.

(c) The third proposal might be that the gene appeared in a small population, where, through the operation of chance (genetic drift) it became established. Ultimately, through population increase and expansion, it might have become distributed among nearly the whole African population, south of the Sahara. No example of a phenomenon of this sort is known, though spread of a harmful gene may occur in rare isolated populations, such as the gene for albinism among the San Blas Indians of Panama. But here, the gene is less harmful than the sicklemia gene, and heterozygotes appear to be quite healthy.

Proposed mechanism of establishment of the gene. From the foregoing brief statement, it is evident that a real problem exists. It should also be clear that, unless one of the proposed mechanisms of establishment of the gene can be demonstrated, a *direct* attack on the problem through the application of the methods of genetic analysis alone cannot afford an answer. It is for this reason that it is here proposed that the disease originated as a by-product of racial mixture. Suggestions of this sort are not without precedent. Haldane ('42) and also Wiener ('43) have proposed that erythroblastosis fetalis is a consequence of the mixture of Rh positive and negative populations. In this instance, of course, the mechanism of production of the disease is different. One may suspect that certain populations are hybrid, because of high variability, as among oaks, but the condition is usually only recognized when the parent races are also identified. Among animals there are numerous instances of genetic disturbance resulting from the crossing of races or species (Dobzhansky, '41, p. 280; and Huxley, '43, pp. 66–68). A particularly relevant example has been studied by Gordon ('51). A fish, *Platypoecilus maculatus* has a sex-linked gene, Sd, which causes black spotting at the base of the dorsal fin due to the presence of macromelanophores in the integument. If a fish carrying the gene is crossed, for example, with another fish, *Xiphophorus helleri,* the F_1 hybrids which inherit the gene Sd may have a larger number of melanophores than the *P. maculatus* parent. This is attributed to melanophore stimulating genes in *X. helleri.* When such F_1 hybrid is backcrossed to *X. helleri* some of the heterozygotes, Sd sd, develop melanotic tumors in the fin region. It is evident that gene Sd may be practically a lethal in the genotype of another species (or genus). The most striking disharmonies resulting from hybridization are observed in crosses between different species, and multiple allelomorph series are usually involved. In race crossing in man, lesser genetic differences are involved. One might therefore, expect the genetic situation to be simpler, and disturbances to be rarer. Gordon refers to other examples of genetic disturbance in hybrids, and notes that C. C. Little has suggested the relevance of hybrid studies to human racial mixture in the United States.

In respect to sickle cell disease, Raper ('50) has expressed the belief "that the appearance of sickle-cell anaemia depends, not only on the extent to which the trait is present in a community, but also on the extent to which admixture with other genetic strains has occurred." He does not attribute the sickling trait to hybridization, but suggests that "some factor, imported by marriage with white persons, is especially liable to bring out the haemolytic aspect of the disease, while the anomaly remains a harmless one in the communities in which it originated." We could accept this statement of the case, if we were sure that, as Raper believes, the trait is harmless in African Negroes. This is discussed below.

In the elaboration of hemoglobin it is most likely that the cooperation of genes at different loci is required. On the basis of this assumption, it is easy to devise a scheme to explain the observed facts. A possible interpretation, which, at least, has the advantage of simplicity, is outlined in the following paragraphs. It may be well to stress the fact that the gene *T* is likely in reality to represent a number of genes, not necessarily all at the same locus. It may be taken to represent the "genotype" of the race.

A majority race, which we shall here suppose to embrace nearly all mankind, including the majority racial element in the Negro, has genotype $\mathrm{S^1S^1T^1T^1}$. The minority race supposedly an early population of a part of Africa, had the genotype $\mathrm{S^2S^2T^2T^2}$. The genic incompatibility is between S^2 and T^1 and *not* between S^1 and T^2. We shall suppose that the sickle cell disease appears in individuals having the genotype $S^2S^2T^1T^1$, where the incompatibility should reach a maximum. The trait, therefore, appears in individuals of the genotypes $S^2S^2T^1T^2$, $S^1S^2T^1T^2$, and $S^1S^2T^1T^1$. At the beginning of race mixture, the $\mathrm{F_1}$ generation would have the genotype, $S^1S^2T^1T^2$. All would reveal the trait. The $\mathrm{F_2}$ generation would contain the following genotype segregants: $S^1S^1T^1T^1$, *2* $S^1S^1T^1T^2$, $S^1S^1T^2T^2$, *2* $S^1S^2T^1T^1$ (trait), *4* $S^1S^2T^1T^2$ (trait), *2* $S^1S^2T^2T^2$, $S^2S^2T^1T^1$ (disease), *2* $S^2S^2T^1T^2$ (trait), $S^2S^2T^2T^2$.

It is at once apparent that when the various trait genotypes are crossed, the observed *disease: trait: normal* ratio of approximately 1:2:1 will usually not be obtained. There will be a deficiency of the genotype for the disease, $S^2S^2T^1T^1$. However, in the American Negro, trait frequency is only 9%. Therefore, the frequency of gene S^2 is low, perhaps 0.05. If we adhere to the hypothesis, T^2 will likewise be infrequent, but may considerably exceed S^2 because of past selection against $S^2S^2T^1T^1$ individuals. This would have resulted in a lowering of S^2 frequency and an elevation of T^2. The consequence is that a considerable majority of trait individuals will have the genotype $S^1S^2T^1T^1$, the other trait genotypes being relatively uncommon. Therefore, most *trait* × *trait* matings will be between the common trait genotype, and the expected 1:2:1 ratio between *disease: trait: normal* will occur in the offspring. Mated with normals, the common trait will give the expected 1:1 ratio. Since $S^2S^2T^2T^2$ normals would be extremely rare, normal parents will very rarely produce trait children.

In those parts of Africa where the frequency of gene S^2 is, by hypothesis, often much greater than it is in America, we may expect the *disease: trait: normal* ratio to differ significantly from that observed in America, particularly in the sense of a decrease in the disease relative to the trait. Beet's work was done among populations for which he reported trait incidences of from 3.5 to 18.9%.

If we suppose that the Negro's genotype was typically $S^2S^2T^2T^2$, we cannot explain why mixture in America with Whites and Indians has evidently reduced trait frequency.

The theory does not explain why gene S^2 has not been largely eliminated by selection. It is likely that the answer to this question is contained in Raper's ('50) observations. His paper is primarily concerned with showing that the introduction of White and American Indian genes into the Negro's gene pool has greatly increased the incidence of the anemia; that in Africa, in spite of the higher trait incidence, the severe manifestations of the disease are rare. Raper states that, in spite of the presence of numerous, often enthusiastic, observers in Africa, only 106 cases of the anemia among natives had then been reported, and that many of these were doubtful. In rebuttal, Neel points out that in Africa, where infant mortality is high, few children with the disease may survive early infancy. Raper further remarks that his thesis "would explain why sickle-cell anaemia has been found relatively commonly amongst Whites, and why amongst Americans the Black Caribs are immune." (The Black Caribs of Honduras are regarded as a practically pure African transplant.) In a later paper Lehmann ('51) gives further evidence to show that the low incidence of the disease in Uganda is due neither to high infant mortality nor to deaths in utero.

According to the present hypothesis a lower incidence of the disease in proportion to the trait would be expected in Africa; yet the absolute incidence probably should not be lower than in America. If Raper's belief is correct, one may suppose that the manifestations of the disease are less severe in the Negro's genotype than in that of other races. The introduction of gene S^2 in Africa, while producing the possibly mild handicap of the trait, rarely causes anemia. The rate of elimination of the gene in Africa may therefore be very low.

Raper further comments that his view "leaves unexplained the fact that mixture of Hamitic and Bantu stocks in Uganda has not resulted in an increase of sickle-cell anaemia there." We may recall that these Hamitic language groups ranged from 0.8 to 3.9% in incidence of the trait. The crucial race mixture in the Hamite may have been so early that gene S^2 has been nearly eliminated by selection. In America, much of the mixture has been very recent in terms of number of generations. In South Africa, where the Bantus have absorbed much Hottentot (or Boskop), according to Coon, Garn, and Birdsell ('50), the incidence of the trait is relatively low.

The theory assumes that in man certain populations had taken a first step in the development of the reproductive isolation which is responsible for species formation. There is no need to go into the manifold implications of the theory in relation to an understanding of the origin of many clinical disorders. If further analysis, observation, or experimentation substantiates the interpretation, it may help in the analysis of the origin of many populations. If the report (Raper, '50) that the highest incidence of the trait occurs in "pygmoids" is confirmed, the mixture responsible for sickle cell disease may be indicated. In a paper the writer has been unable to consult, the Scientific American (May, '52) states that Lehmann and Cutbush found 3.3% of sicklemia in the Todas of India and 30% in the Irulas, the Badagas being intermediate. These percentages agree with the relative proportions of Negrito which might be inferred on other grounds, and suggest that the mixture is not of great antiquity.

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DIAL SPREADING CALIPER

NATHALIE F. S. WOODBURY

University of Kentucky

TWO FIGURES

Dr. Charles E. Snow of the Department of Anthropology and Dr. Karl O. Lange of the Department of Physics, both of the University of Kentucky, have produced another anthropometrical instrument in a rapidly growing series. This is the dial spreading caliper illustrated below.

Spreading calipers have been in use for many centuries with numerous variations in the shape and position of the scale. Their use in anthropometry dates from the first half of the 19th century. Lucille Hoyme highlights the history as follows: Parchappe in 1836 used a spreading caliper for cranial measurements. However, those currently in use are Martin's and Hrdlička's modifications of an adaptation of Baudeloque's obstetrical pelvimeter described by Broca in 1865. (Personal communication.) The Kentucky caliper is unique in using a dial rather than a bar to record the spread. For this reason and because lightweight materials have been used in its construction, the caliper is easier to read and handle than those of traditional style. The dial case and tapered arms of the instrument are of aluminum, the latter tipped with knobs 11 mm in diameter. The dial gears are

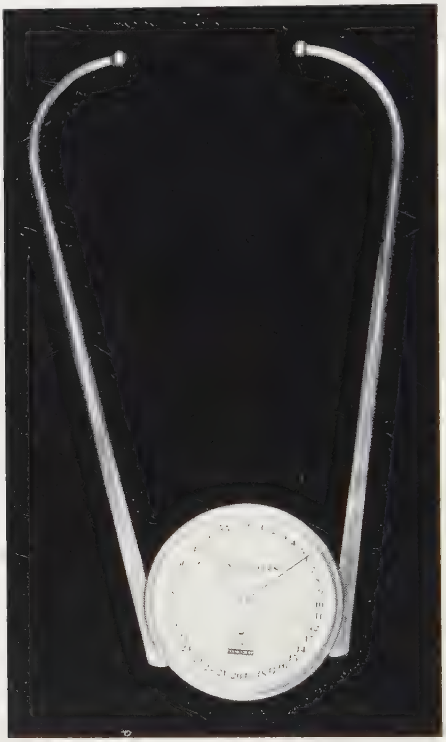


Fig. 1 The dial spreading caliper open.

bronze and the face is aluminum with a baked-on white finish on which the numerals and calibrations are marked in black. The dial face is covered with a flat, unbreakable plexiglass crystal. The left arm is mobile while the right is stationary in relation to the dial case. However, the same technique of handling is used in measuring as with the bar-type spreading calipers. The dial is calibrated to 33.5 cm in a single revolution, but the total spread of the tips is 35 cm. When closed the overall length of the instrument is 23.7 cm and the greatest width is 8.5 cm. The dial case is 6.9 cm in diameter and 1 cm thick. Since the caliper is both compact and light, weighing only 138 gm, it can be carried comfortably in the pocket, handy for immediate use.

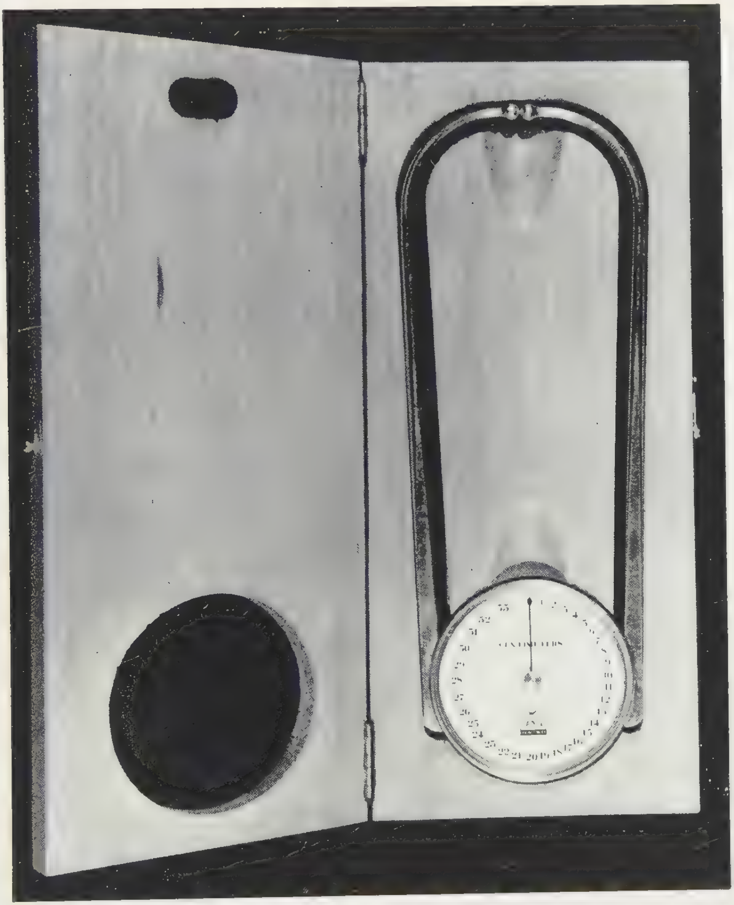


Fig. 2 The dial spreading caliper boxed.

The instrument is boxed in a hinged wood case, 10 3/8 in. long, 4 1/2 in. wide, and 1 1/4 in. thick. The caliper fits snugly into felt-lined cut-out areas. This prevents shifting. Two sloping cuts make removal easy.

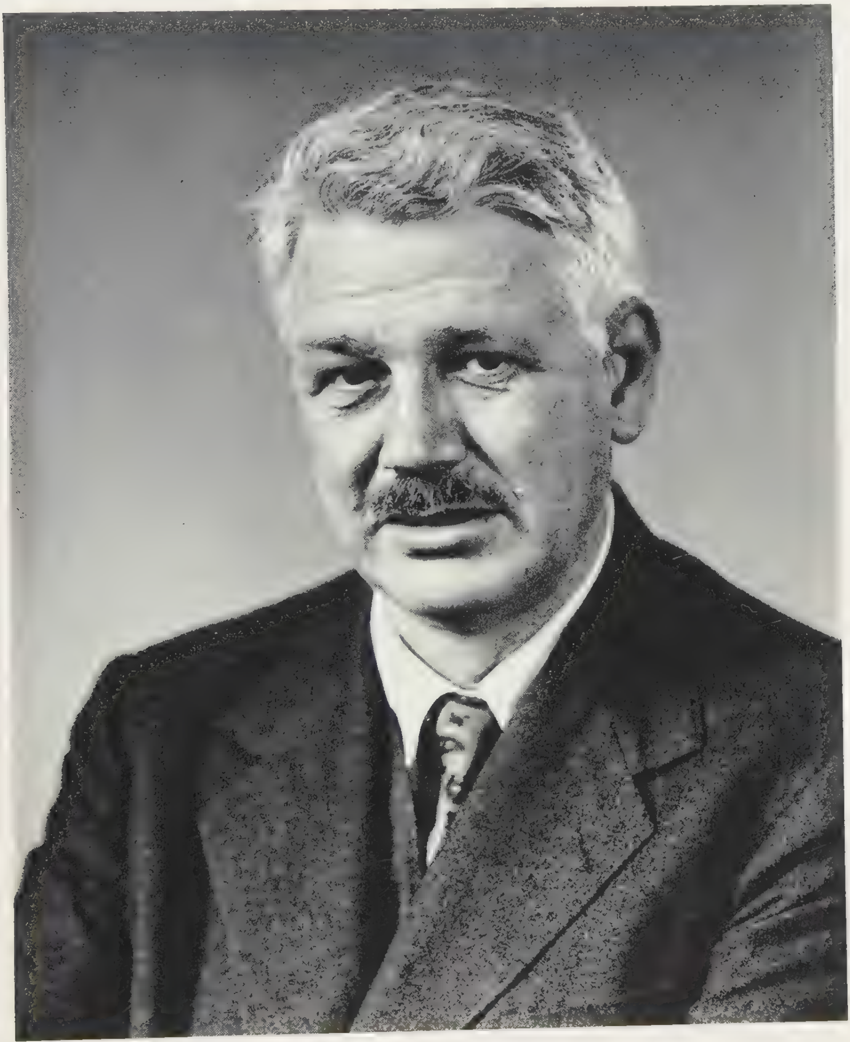
The dial spreading caliper is manufactured by Dr. K. O. Lange, Instruments, Route 5, Richmond Road, Lexington, Kentucky. The list price is $62.50.

MORPHOLOGICAL EVOLUTIONARY SEQUENCES.— If the derivation of the bicuspid anterior lower premolar of the Hominidae from the predominantly unicuspid type characteristic of the anthropoid apes is accepted, and if it is also accepted that the small canine of modern Man is the result of a secondary retrogression, there appears to be no inherent objection on the basis of the palaeontological evidence to the derivation of the Hominidae from a group of Miocene or Pliocene apes not very different from some of those already known. Indeed, it is possible on the evidence now available to envisage an approximate morphological sequence leading from types such as *Parapithecus* and *Propliopithecus* to *Limnopithecus* and *Pliopithecus*, from primitive anthropoid apes such as *Proconsul* to the various genera of the Dryopithecinae, and from a progressive dryopithecine type such as *Ramapithecus* to the precursors of the Hominidae. It is not to be supposed, of course, that this represents an actual evolutionary sequence — it is no more than a tentative morphological sequence, and a very approximate one at that. But the very fact that it is now possible to suggest a graded sequence of this kind is an encouraging sign of the wealth of material which is now beginnnig to accumulate in relation to the early palaeontological history of the Hominoidea.— W. E. LeGros Clark. New palaeontological evidence bearing on the evolution of the Hominoidea. Quart. J. Geol. Soc. London, vol. 105, pt. 2, no. 418, 1950, pp. 225–264.

NOTE

VIKING FUND MEDALIST FOR 1951

The American Association of Physical Anthropologists has selected Dr. Carleton Stevens Coon of the University of Pennsylvania as the person who made the most outstanding achievements in physical



CARLETON STEVENS COON

anthropology. The Awards Committee, Doctors Hooton, Stewart, and Krogman, Chairman, overwhelmingly voted for Doctor Coon. The action of the Awards Committee was unanimously approved by the Executive Committee of our Association.

Doctor Coon published two books during the year of the award. *The Mountains of Giants* is an important monograph on the Ghegs of Northern Albania. It elucidates the problem of the Dinaric race, showing that many of the traits are due to cradling and some to age. The careful definition of the populations and consideration of social and environmental factors, the use of migrants as controls, and the detailed demonstration of the way one measure varies as a function of another; all these make this a major contribution in the very best anthropological tradition. In *Races,* a popular account written with Doctors Garn and Birdsell, Doctor Coon outlines possible adaptive explanations for many of the races of man. Professor Dobzhansky says that this book may have inaugurated a new era in racial studies.

The choice of Doctor Coon honors not only these two publications but also his many other contributions to anthropology as teacher, ethnologist, archeologist, finder of fossil men, and author of numerous other anthropological papers and books. Doctor Coon's major contribution to physical anthropology is *The Races of Europe.* Dr. T. D. Stewart wrote to me, commenting on the choice of the committee, that, when we want to know about Europe, we usually find that Doctor Coon has the answers. He also mentioned Coon's excellent use of English and how much other physical anthropologists would profit if they could make their reports equally readable. Although primarily a physical anthropologist, Doctor Coon has always had the widest interests. His books of general anthropological importance include: *Tribes of the Rif,* (with Doctor Chapple) *Principles of Anthropology,* and *A Reader in General Anthropology.* At a more popular level he has written numerous tales of science and adventure, based on his trips to North Africa. These include: *Flesh of the Wild Ox, The Riffian,* and *Measuring Ethiopia and Flight into Arabia.* His most recent book, *Caravan,* aims to help the American to understand the peoples of the Near East, as does Coon's article in Professor Ralph Linton's *Most of the World.*

Right now Doctor Coon is producing archeological monographs and finding more fossil men. Doctor Hooton describes him as a brilliant student and an inspiring teacher, an original and productive scholar. With Hooton's judgment we may all agree, and add, "A wonderful guy." On behalf of the American Association of Physical Anthropologists, it is a pleasure to present Dr. Carleton Stevens Coon.

S. L. WASHBURN
President

REVIEW

THE BIOLOGY OF HUMAN STARVATION. By Ancel Keys, Josef Brožek, Austin Henschel, Olaf Mickelsen, and Henry Longstreet Taylor; with the assistance of Ernst Simonson, Angie Sturgeon Skinner, and Samuel M. Wells. In two volumes. xxxii + 1385 pp.; 158 figs.; 565 tables. Univ. of Minn. Press, 1950. $25.00.

It is a rare intellectual treat to read a report so carefully planned, so well prosecuted, so cautiously interpreted, and so well presented, as the "Minnesota Experiment." In all fairness to the authors and to myself I must state that my study and my review cover, in vol. I, "Background" (pp. 1–80) and "Morphology" (pp. 81–288). I did not read "Biochemistry" (pp. 289–574) and "Physiology" (pp. 575–763). In vol. II I read "Growth and Development" (pp. 974–1001) and the "Appendixes" (pp. 1069–1254). I did not read "Psychology" (pp. 767–920), and "Special Problems" (pp. 921–973 and 1002–1068). What I read has, I feel, a direct relation to techniques and observations in physical anthropology.

The basic problem in this study was the effect of controlled semistarvation upon body structures and tissues. The voluntary subjects were 32 young adult males who chose thus to serve their country in wartime. Their average age, as of 1-1-'45, was 25.5 years (S.D. 3.47, range 20–33); average height was 178.8 cm (S.D. 5.77, range 167.5–191.9); average weight was 69.39 kg (S.D. 5.85, range 62.0–83.6); average W/H index was 38.8 (S.D. 2.8, range 35.0–54.3). The sample compared favorably in these, and other, variables with the averages of 99,172 selectees of World War II (table 14, p. 67).

The "Minnesota Experiment" covered a main period of 11-19-'44 to 10-20-'45, with two follow-up periods of 3-18-'46 to 3-23-'46 and 8-26-'46 to 9-14-'46. Within the main period there was a 12 weeks' control period, a 24 weeks' semistarvation period, and a 12 weeks' "restricted" and an 8 weeks' "unrestricted" rehabilitation period. Anthropometric measurements and photographs were taken at frequent and regular intervals. Weights were taken daily, but given as weekly averages. The following measurements were taken: height, weight, sitting height, bi-deltoid diameter, bi-acromial diameter, bi-cristal diameter, bi-trochanteric diameter, transverse and anteroposterior diameters of the thorax, and circumferences of thorax, abdomen, upper arm, thigh, and calf. Skin-fold thickness data on chest, abdomen, and lateral thigh were gathered from the literature. Linear dimensions were to the nearest 1/16 in., calipers to the nearst 1 mm. All measurements were taken standing, except sitting height and limb circumferences; they were done in the morning and were done in duplicate. As much as was possible measurements were based on the skeletal framework; in addition the measurements offered estimates of overlying soft tissues. The photographs were measured as in the Sheldon technique (see Lasker, AJPA, 5 (n.s.): 323–333, 1947). All anthropometric measurements and specific gravity determinations were done by Brožek.

Persons who starve lose weight; when they eat again they gain weight. Where does the weight loss localize? how? how rapidly? Where does recovery localize? These are some of the questions which faced the investigators. The answers were far from simple, for weight is a complex measurement. In starvation the cheekbones become prominent, the neck thin and "long," the clavicles stand out in relief, the shoulder girdle loses padding, the ribs are prominent, the scapulae are "winged," the pectoral area is flat, the vertebral column is prominent, the waist is "pinched," the buttocks are flat and the iliac crests "stick out," and the arms and legs are "spindly." Obviously weight loss means dimensional loss (a quantitative loss). It was found that circumferences decreased more than linear dimensions; of the former, thigh circumference lost the most absolutely, upper arm circumference the most relatively. The "true changes" in soft tissue were calculated as follows:

> "In order to obtain an indication of the true changes in the soft tissues we should correct the gross values of the circumferences of the limbs for the bony component of the measurement. It has been possible to do so for the thigh. The dimensions of the femur were obtained from x-ray pictures of the leg, taken in a standard dorso-ventral and medio-lateral orientation and at a standard distance (30 in.) of the x-ray tube from the film. At a level corresponding approximately to the height of the leg at which the thigh circumference was determined, the average measured values were 32.4 mm for the anteroposterior diameter of the femur and 29.7 mm for the lateral diameter. These values were corrected for magnification resulting from the facts that the x-rays were not parallel and that the sagittal and coronal plane of the femur did not coincide with the plane of the film; in making the correction for magnification, both the focal-film distance and the object-film distance were taken into account. The percentage of distortion has been obtained by interpolation from Files' table.
>
> "By subtracting the value for the area of the femur from the gross cross section of the thigh we obtain the cross-sectional area of the soft tissues. The absolute differences between the control period and the successive stages of semi-starvation will remain, of course, the same. However, with a decrease in the

> denominator (C) the percentages are increased; the larger the cross-sectional area of the bone in relation to the surrounding soft tissues the larger will be this difference. The actual decrement in the cross-sectional area of the thigh, corrected for the bone, was 27.80% at S12 and 36.40% at S24. Using 3.8 cm as the estimated value for the cross-sectional area of the humerus at the approximate level at which the circumference of the arm was measured, the semi-starvation decrements were 34.8% at S12 and 45.6% of the control value at the end of the semi-starvation period'' (p.135).

The authors have developed a method of predicting the weight loss curve for each subject in the experiment: $W_x = W_f + K(24 - t)^2$, where W_x = weight at t time, W_0 = initial weight, W_f = final predicted body weight, P = desired total per cent loss of body weight, t = time in weeks, and $K = W_0/100 \times P/24^2$.

It soon was apparent that weight loss was largely fat loss, with factors of body hydration also to be considered. In recovery, fat gained more than muscle. It is necessary, in analyzing fat loss (or gain), to differentiate between stored or deposited fat (largely subcutaneous) and functional fat (in the structure of active cells). In the normal young adult male fat is 13–14% of body weight (a bit more in the female); at least half of this fat is subcutaneous in position. The relative amount of body fat in a given individual may be reckoned as follows: per cent body fat $= 100\left\{\frac{5.548}{\text{sp. gr.}} - 5.044\right\}$; it may also be expressed as $\frac{M_f}{W} = \frac{5.548}{G} - 5.044$, where M_f = weight of fat, W = gross body weight, G = specific gravity.

The effect of starvation on bones and teeth is presented, for the most part, from experimental evidence on non-human mammals. Work on cats (by other and earlier investigators) showed gross weight losses of 33–55%, while skeletal loss was only 6–19%. Mineral losses in bones and teeth were: phosphorus 5.5%, calcium 7.8%, and magnesium 8.3%. In humans it is notable that dental disease (caries) is less frequent in semi-starved populations. The DMF Index (decayed, missing, filled) at the end of World War II was 1.05 for Italian children aged 11–15 years, 4.66 in U. S. children; in Italian adults, aged 51–60, it was 10.80, in U. S. adults of like age it was 23.20. Bone density was studied in the Minnesota subjects via the Mack-Brown Electronic Densitometer. In 24 weeks of semi-starvation, with 24% of body weight loss, there was no decrease in the density of the bone tested (phalanx II, of finger V). In organs and tissues there is differential loss (based on the literature). Muscle loss is proportionate to the body weight loss as a whole; individual muscle fibers decrease in size; liver and intestines are next in per cent of loss, then heart and kidney; brain and cord lose very little.

One of the more significant chapters, in the sense that it stresses the functional aspect of morphologic analysis, is "Compartments of the Body." Here lies, I think, a basic contribution to methodology, in that weight — too often handled as a discrete or unitary measurement — is really broken down into basic components.

In a normal adult male the bony and cartilaginous skeleton comprises 15–20% of the total body weight; of this mineral ash makes up about 4% of the total body weight. The plasma and total blood volume form another compartment, little affected by semi-starvation. The extracellular fluid is an important compartment, "because it is presumably this compartment which undergoes the major changes when alterations in hydration occur" (p. 277). The correlation between intravascular and extravascular hydration is + 0.706. Other compartments, already mentioned in this review, are total fat (via the specific gravity method) and the active tissues ("the difference between the body weight and the sum of the fat plus the thiocyanate space [extracellular fluid-space], plus the bone minerals" p. 283). This would include the red blood cells, or most of their mass.

The concept of "compartments" is a functionally dynamic one that we physical anthropologists may well adopt — and adapt. It implies the answer to: "What are we *really* measuring?" or, "What does the measurement tell?" with respect to a specific problem. Technique must give way to analysis and interpretation, *a priori* rather than *a posteriori.* To an extent many of us have come to this conclusion in our own work, as, for example, the rejection of total facial height (nasion-gnathion) as a unit dimension. We may have long realized that weight is a hybrid dimension, that it is inherently variable, that it is age- and sex-linked, and so on, but we have gone blithely ahead with our multifarious (and often nefarious) H–W ratios. Apropos of this stricture the authors conclude that "the technique of somatotyping would appear to be more useful for determining the state of nutrition than for determining the inherent constitution" (p. 153).

The effect of semi-starvation on growth and development is thoroughly surveyed in vol. II. There is no influence on the duration of pregnancy. Labor averages 5 hours shorter in better-fed mothers, and congenital malformations are more frequent in poorly-fed mothers (especially 5–8 weeks post-conception). The amount, timing, and rate of post-natal growth in height and weight are adversely affected.

The two-volume work is concluded with 4 extensive appendixes: I, Methods (pp. 1071–1126); II, Detailed Data from the Minnesota Experiment (pp. 1127–1246); III, Wartime Diets and Rations (pp. 1247–1248); IV, Some Notable Famines in History (pp. 1249–1252).

There is an exhaustive "References" (pp. 1255–1342). In all major respects "Human Starvation" is a noteworthy achievement. For our own specialty, physical anthropology, it represents an opportunity and an example of anthropometric techniques put to work. But it represents more than that: it represents opportunity for the future: that physical anthropology, as a specialty, make its aims, its skills, its achievements, its hopes and aspirations, known to an ever-widening circle within the realms of functional morphology and physiology.

As I studied these volumes I thought again and again, "They borrowed our techniques, and gave us in turn ideas and concepts of the functional meaning and application of those techniques."

Our bread was cast on the waters of the "Minnesota Experiment" — and this magnificent report came to our shores. We give thanks.

WILTON MARION KROGMAN
Graduate School of Medicine
University of Pennsylvania

PEDIGREES OF NEGRO FAMILIES. By R. RUGGLES GATES. vi + 267 pp. The Blakiston Company, Philadelphia, 1949. $5.00.

The present volume is a collection of over 200 genetic pedigrees of Negroes, particularly from North America, Puerto Rico, and the British islands of the Caribbean. These pedigrees were largely gathered by Negro students of Professor Gates at Howard University from their own relatives and friends. The style of presentation is quite similar to that used in Gates' "Human Genetics" (New York, 1946). Both works are organized under the rather arbitrary headings of abnormalities, physiologic and constitutional conditions, normal and abnormal nervous and mental conditions, and racial characters.

Most of Gates' Negro pedigrees are based on hearsay evidence. In such material, minor abnormalities are relatively apt to be overlooked by inexperienced observers. Pedigrees from students may therefore be incomplete. Inconsistencies in occasional pedigrees may also remain unresolved.

Despite these limitations, student pedigrees are by no means worthless documents for research in human heredity. Moreover, some families may be more likely to divulge their abnormalities to a student member of the group than to an outsider. In general, student pedigrees seem especially useful when they form part of a considerable mass of evidence on some consistent pattern of inheritance of a relatively common, conspicuous, and strongly gene-influenced trait.

Gates' pedigrees indicate that the most prevalent mechanisms of inheritance of a number of human traits may be similar in Whites and Negroes. In particular, autosomal dominance with incomplete penetrance may be the usual mode of inheritance of the factor for ulnar polydactylism in both races.

Some of Gates' Negro pedigrees deal with the inheritance of stature, obesity, cephalic index, hairform and skin color. These findings are unfortunately based to such an extent on the observations of laymen that their exactness is open to doubt. Gates has therefore gone only a little further than earlier students such as Davenport. The study of probable multifactorial inheritance, in my opinion, requires extreme precision in the descriptions of phenotypes, far beyond most of the observations of laymen, geneticists, or even physical anthropologists. Gates suggests, for example, that skin color may be inherited on the basis of three factors rather than Davenport's two. This hypothesis calls for much more exact determinations of skin color than Gates had carried on when this book was written.

At present, Gates is at work on the inheritance of skin color as measured by spectrophotometry. This effort is eminently worth while. If he succeeds in this endeavor, it may well be regarded by future investigators as his finest contribution to anthropological genetics.

EDWARD E. HUNT, JR.
Forsyth Dental Infirmary for Children

PROCEEDINGS OF THE TWENTY-FIRST ANNUAL MEETING OF THE AMERICAN ASSOCIATION OF PHYSICAL ANTHROPOLOGISTS

The twenty-first annual meeting of the American Association of Physical Anthropologists was held on Friday, Saturday, and Sunday, March 21, 22, and 23, 1952, at the New York Academy of Sciences, 2 East 63rd Street, New York City 21, at the invitation of Hunter College of the City of New York and the Wenner-Gren Foundation for Anthropological Research. One hundred and thirty-seven people attended the meetings, including 73 members of the Association.

At three o'clock on Friday afternoon in the Hunter College Playhouse, President George N. Shuster of Hunter College welcomed the Association cordially, and pointing to the vistas being opened up by anthropology introduced the 1952 Wenner-Gren Foundation lecturer, Dr. Glenn L. Jepsen. Dr. Jepsen, Curator of Vertebrate Palaeontology at Princeton University, spoke and showed motion pictures under the title "The buffalo kill" describing a site in Wyoming's bighorn basin where remains of bison slaughtered for food by latest Pleistocene hunters (Yuma culture complex) allowed a penetrating reconstruction of the ecology of early hunting peoples and an exposition of excavation techniques. A tea and a tour of the Physical Anthropology Laboratories of Hunter College followed. After dinner the business meeting was immediately followed by the smoker in the rooms of the New York Academy of Sciences, allowing those newly elected members of the Association who were present to meet their electors.

The whole of Saturday, March 22, was given over to the Symposium on the Back, organized by Dr. E. Carl Sensenig of the University of Alabama School of Medicine, and sponsored and generously supported by the Wenner-Gren Foundation for Anthropological Research. Ninety-two members and guests attended the annual dinner on Saturday evening, which was followed by an address by Dr. Harry L. Shapiro on "the UNESCO Statement on Race." Sunday's program was featured by a picnic lunch, prepared personally by Mrs. Eunice T. Miner, Executive Director of the New York Academy of Sciences, for the seventy people who had braved the storming rain to attend the meetings.

BUSINESS MEETING, MARCH 21, 1952

The President of the Association, Dr. S. L. Washburn, announced that the Wenner-Gren Awards Committee for 1953 would consist of two previous winners, Dr. C. S. Coon, and Dr. W. K. Gregory, plus Dr. T. D. McCown. Dr. W. W. Howells as Editor of the Journal was appointed to the Nominating Committee, with Dr. W. L. Straus (chairman) and Dr. R. White.

The minutes of the twentieth meeting of the Association as recorded in the Proceedings were voted approved.

The Secretary pointed to the acceleration in the rate of increase in membership: (1930) 109; (1942) 153; (1946) 183; (1950) 219; (1951) 245.

The Executive Committee proposed 59 candidates for membership, who were unanimously elected:

Robert M. Adams, Jr.
William R. Adams
Bert W. Aginsky
Ethel G. Aginsky
Joseph Auer
David A. Baerreis
N. A. Barnicot
Marston Bates
Harry E. Blomquist
Kurt Bröste
Josef Brozek
Joseph Buchman
George F. Carter
William Caudill
Charles E. Clauser
John D. Ellis
Irvin Emanuel
William J. L. Felts
Ernestine Friedl
Charles E. Gillette
Bentley Glass
Martin Gusinde
Don J. Hager
Alfred R. Hale
Gerald Henderson
Göran Hjelmman
Dorothy C. Jensen
Tracy Lay
E. A. Lusterman
Maury Massler

Emil M. Misura
Melvin L. Moss
Léon Muller
Martti J. Mustakallio
Donald Opdyke
Frank Pauls
Niilo N. Pesonen
George Philippas
William Pollitzer
Froelich Rainey
J. T. Robinson
Robt. G. H. Robinson
Chandler W. Rowe
L. D. Sanghvi
Bhim Savara
Paul B. Sawin
Bobb Schaeffer
Ronald Singer
Božo Škerlj
R. Dale Smith
L. W. Sontag
Antti J. Telkkä
Selma Ø. Thomsen
William Townsley
James van Stone
Franklin D. Van Wart
Roger G. Wasser
William H. Womble

In order to simplify the work of the Executive Committee in receiving names and qualifications of candidates for membership and to encourage membership among interested graduate students and among workers in fields adjacent to physical anthropology, the Executive Committee proposed changes in the wording of the By-Laws relating to membership (Article V). It was decided to circulate the rewritten proposal among the members either in the Journal or by letter, to prepare for a formal change in the By-Law at the next meeting.

The reports of the Treasurer and of the Auditing Committee were read and accepted.

TREASURER'S REPORT

Prudence Bond			$ 27.50
U. S. Savings Bonds (maturity value)			3,000.00
			$3,027.50
Bank balances, March 15, 1951		$2,300.98	
Receipts:			
Dues	$1,753.00		
Foreign postage and refund	9.00		
Sale of "STUDIES IN PHYSICAL ANTHROPOLOGY"	24.00		
Gift from Dr. R. J. Terry	100.00		
Grant for Symposium on the Back, from Wenner-Gren Foundation, March 22, 1952	700.00		
Interest on savings account	8.25		
		$2,594.25	
			$4,895.23

Expenditures:			
To Wistar Institute (subscriptions)	$1,310.00		
To Wistar Institute (proceedings)	42.74		
To Wistar Institute (programs)	67.15		
To Bertram Kraus (Newsletter)	50.00		
To F. P. Thieme (guests at annual dinner)	14.70		
Secretarial expenses	155.54		
Express and postage for foreign members	9.34		
Bank charges	4.90		
		$1,654.37	
Bank balance, March 18, 1952		$3,240.86	
			$4,895.23

March 15, 1952
Respectfully submitted
J. LAWRENCE ANGEL, Treasurer

Certified to be as stated above.
March 21, 1952
FRED P. THIEME
RICHARD H. OSBORNE
Auditing committee

The Treasurer took over the Association's books on November 1, 1951, being appointed by the President, Dr. S. L. Washburn, after the resignation of Dr. Earle L. Reynolds, who was called to Japan to do research for the government. The Treasurer expressed thanks to Dr. Reynolds, Dr. Lasker, and Dr. Washburn, all former Treasurers, for advice.

The Executive Committee raised the question of possible incorporation of the Association as a non-profit society, especially with the increasing membership. Dr. H. L. Shapiro, seconded by Dr. N. C. Tappen, moved that the Association incorporate after an investigation by the Treasurer into the means and costs of such action in the state of Pennsylvania or other states. The Executive Committee, stimulated by the gift of Dr. Terry, suggested that an endowment fund be set up as soon as the present Bonds mature and that a Contributors' Fund be established immediately to receive special gifts of any amount. Interest from the endowment fund could be used to support and encourage publication of original work by students. No change was proposed in the budget, in hopes that the increased membership would pay for the corresponding rise in costs of mailing and printing.

The Editor of the Journal, Dr. W. W. Howells, read the following report:

REPORT OF THE EDITOR

Volume 9 of the Journal contained 484 pages, made up of 35 articles and brief communications and 5 book reviews. During the year, 14 articles submitted were not accepted for various reasons. During the past summer, the Editor began an exchange of letters with the Associate Editors which lasted over some months, starting originally from the Editor's feeling that the members of the Association themselves have not been sending articles to the Journal, and so not allowing the Journal to represent physical anthropology as really up-to-date compared to other fields of anthropology and biological science. Other matters of policy came under discussion, and the major conclusion arrived at was that all the Associate Editors and also all officers of the Association should try to stimulate general interest in the Journal as a medium for reports on a wider variety of topics and present concepts of physical anthropology, and should also try to procure specific articles on known work in process when possible. At the instigation of President Washburn, other matters relating to the Journal are under consideration by the Executive Committee.

In any case, the Journal gives signs of steady improvement. The number of members of the Association contributing to volume 9 rose this year, constituting 23 out of 39 different authors appearing in the volume, several of them contributing more than one article. At the same time, a number of good articles were received from other countries, preserving or enhancing the international nature of the Journal. The increasing breadth of authorship is due in part to the efforts of the Associate Editors, and the Editor feels that it is also due in part to a constantly increasing breadth of interest in the Journal both within and without the profession, although it is difficult to demonstrate this in figures.

The Editor's report was moved accepted by Dr. S. M. Garn, seconded by Dr. R. W. Ehrich.

President Washburn discussed briefly the Association's relations with the American Anthropological Association, stressing the needs for general papers in Physical Anthropology in the *American Anthropologist,* for increased circulation for the American Journal of Physical Anthropology, for use of the A.A.A's facilities for finding jobs, for interest in the Symposium on Physical Anthropology at the Philadelphia meetings of the A.A.A. at Christmas, 1952, and for action to accomplish these ends. Drs. Washburn and Howells pointed out that the Inter-Society Committee, composed of the Secretaries of the component associations, was the channel of communication with the A.A.A.

The Nominating Committee, W. W. Howells, chairman, J. B. Birdsell, and T. D. Stewart, proposed the following candidates:

For Vice-president, Dr. Mildred Trotter; for Secretary-Treasurer, Dr. J. Lawrence Angel; for Executive Committee, Dr. Charles W. Goff; and for Associate Editor, Dr. T. D. McCown. The candidates were elected unanimously.

The Executive Committee expressed its feeling that as the Association expands the Committee runs the risk of not being sufficiently representative. Dr. C. Wesley Dupertuis moved that a second office of Vice-president be created. With one dissenting vote it was decided to act at the next meeting on this proposal to change again Article II, Section 1 of the By-Laws after circulating the proposed change among the membership.

The Secretary read a letter from Dr. Earle Reynolds describing a conference of outstanding Japanese workers in growth and development to be held at Hiroshima under the auspices of the Atomic Bomb Casualty Commission to discuss ideas and methods in the area of human growth current among workers in Japan and America.

With reference to the calendar of the Association for the year the next annual meeting was proposed tentatively for the University of Chicago during Easter vacation, 1953. Yellow Springs or Columbus, Ohio were also discussed as possibilities. Members were reminded of the Symposium on Physical Anthropology being organized by Dr. William Laughlin of the University of Oregon, Eugene, Oregon, as a session of the Christmas meetings of the American Anthropological Association to be held at Philadelphia in December, 1952, offering another possibility for the annual meeting. It was pointed out that the Newsletter would be better received in October than at the time of the annual meeting. Dr. S. M. Garn moved that $50 be voted for this purpose to be assigned to the Editor of the Newsletter, Dr. Bertram Kraus, University of Arizona, Tucson.

The Secretary proposed a vote of thanks to the Wenner-Gren Foundation and its Director of Research, Dr. Paul Fejos, for making possible Saturday's program including the Symposium on the Back and the Annual Dinner, and for supporting the second issue of Studies in Physical Anthropology, to appear later during the year. This will consist of the work of E. A. Hooton and C. W. Dupertuis on the Irish people.

President Washburn announced that because of the International Conference in Anthropology sponsored by the Wenner-Gren Foundation for the coming summer there would be no Summer Seminar in Physical Anthropology. He stressed his hopes for 1953, pointing out the benefits for the science if the Seminar were organized each year by different groups of people and held in different centers. Dr. W. M. Krogman and Dr. T. D. McCown (both absent) were quoted as interested in organizing seminars dealing with growth of the face and with fossil man respectively.

Thus the Yearbook is now formally separated from the Seminar and becomes an organ of the Association. The Secretary pointed out that the production and free distribution of the Yearbook to Association members and interested people of all nationalities at a cost to the Wenner-Gren Foundation of about $2.00 per copy has done more to benefit and advance Physical Anthropology and the Association than any other single thing (noting that the Journal appears quarterly and is thus multiple). Dr. C. W. Dupertuis moved that whereas the

Association recognizes the value of the Yearbook to its members and to the science of Physical Anthropology, the Association express its deep appreciation to the Wenner-Gren Foundation for its support and thanks to Dr. G. W. Lasker for his unflagging work as Editor of the Yearbook. The motion was seconded by Dr. E. E. Hunt and passed unanimously.

The Executive Committee asked the President to express the gratitude of the Association to one of its founders, Dr. Terry, for his gift and valuable proposals for the Association's welfare.

The Resolutions Committee submitted the following report:

REPORT OF THE RESOLUTIONS COMMITTEE

1. Be it resolved that the Association hereby expresses its deep appreciation to Mrs. Eunice T. Miner for her help in the arrangement of these meetings and to the New York Academy of Sciences for its cordial hospitality and for the use of its facilities.

2. Be it also resolved that the Association expresses its very real gratitude to Dr. Dorothy Cross Jensen, Chairman of the Department of Anthropology of Hunter College, to Professor Elsie Steedman, and to the Anthropology Club of Hunter College for their work in arranging these meetings.

3. Be it further resolved that the Association also expresses its warm thanks to President George Shuster of Hunter College and to Hunter College for the use of the Hunter College Playhouse and for their cordial reception of the members of the Association.

4. And be it further resolved that the Association also expresses its deep sense of obligation to the Wenner-Gren Foundation for Anthropological Research as a consistent benefactor to physical anthropologists and specifically on this occasion for its sponsorship of the second volume of *Studies in Physical Anhropology,* for the Sixth Summer Seminar for Physical Anthropologists held under its auspices and with its financial support, and for its moral and financial support of the Symposium on the Back being held at these meetings.

5. Be it also resolved that the Association extends its thanks to Dr. Carl E. Sensenig for his initiative and for his painstaking work in the organization of the Symposium on the Back which is a vital part of the program of these meetings.

6. Be it still further resolved that the Association herewith voices its sense of grateful recognition to Dr. Gabriel Lasker for his unflagging, skillful, and successful efforts in the editorship of the

Yearbook of Physical Anthropology which under his hand has become an outstanding contribution to the field.

7. Be it also resolved that the Association expresses its thanks to Prof. Robert J. Terry of Washington University for his gift of $100.00 to the Association Contributors' Fund.

8. And be it resolved that the Association hereby expresses its sense of the loss of a valued colleague in the death of Dr. Robert Broom of South Africa, and that the secretary be instructed to send a letter of condolence to the Transvaal Museum at Pretoria on behalf of the Association.

9. And be it finally resolved that these resolutions be incorporated in the minutes of these meetings and that they be published in the Proceedings of this Association.

Respectfully submitted,
C. W. Dupertuis
R. W. Ehrich

These resolutions were passed unanimously.

Under new business, there was brief discussion of a possible new name for the Yearbook, and a further expression of the thanks due to Dr. Lasker for his efforts in organizing and editing the six issues which have appeared so far (1945–1950, reviewed in 1946–1951).

The scientific part of the program of the meeting included the following papers:

REGULAR PROGRAM

1. *Temperature variation and human efficiency.* R. E. G. Armattoe; Lomeshie Research Centre, Kumasi, Gold Coast.

(Full paper not read.)

2. *The design of sculptured headforms from measurements in three dimensions.* Edward E. Hunt, Jr., Forsyth Dental Infirmary for Children, Boston, Massachusetts.

In designing protective masks which seal around the periphery of the human face, an important adjunct is a sculptured head whose measurements reflect the dimensions of the population to be fitted. The most useful heads are those which reflect metrical central tendencies in the population of potential mask wearers, which can be defined as "harmonic" headforms.

Plans are under way for the construction of harmonic headforms, which can be used for Americans more than 5 years old, in which the metrical standard is face height (nasion-gnathion) in regular increments. A few simplifying assumptions concerning variations due to growth, race and sex make it possible to make about 14 harmonic headforms representative of this entire population.

The most suitable measuring instrument for designing sculptured headforms is one which locates points on the surface of the head and face in three Cartesian coördinates. Such an instrument was used during World War II by the Chemical Warfare Service of the U. S. Army. An improved model is currently being built and will soon be used for mass anthropometric surveys by anthropometrists of the Forsyth Dental Infirmary.

3. New anthropometric measurements used in the air force. 1/Lt. Franklin D. Van Wart, Anthropology Unit, Aero Medical Laboratory, Wright Air Development Center, Dayton, Ohio.

The needs of Air Force anthropometry have been expressed in a series of new measurements necessitated by problems peculiar to adapting aircraft and personal flying equipment to the man.

Functional landmarks rather than landmarks based solely on skeletal or other precise anatomical points have been considered in these new measurements. The accented use of cranial arcs rather than classical diameters has been incorporated. More attention has also been paid to the measurements of man in a seated functional position than has been the case in previous anthropometric surveys.

The requirement of closer tolerance of fit without compromising mobility of the man has suggested either modifications of measurements previously used or the development of new specific measurements.

4. A nomograph of hand measurements. 2/Lt. Gilbert S. Daniels, Anthropology Unit, Aero Medical Laboratory, Wright Air Development Center, Dayton, Ohio.

A nomograph of hand measurements has been prepared from data gathered on a series of more than 4,000 Air Force flying personnel during the 1950 Air Force Anthropometric Survey. Hand breadth at *metacarpale* (across the metacarpal-phalangeal joints of the second through fifth fingers) and hand length have been used as independent variables. Palm length, length of the first phalanx of the third finger, hand breadth across the thumb, maximum circumference of the third finger and thickness of the hand at the metacarpal-phalangeal joint of the third finger are the dependent variables presented in the nomograph.

Although the correlation of each of the separate dependent variables with its more closely related independent variable is only very slightly increased by the combined use of both independent variables, the material is presented as a nomograph based on two independent variables in the hopes of simplifying the presentation and facilitating the practical use of correlation data.

5. Gripping strength and efficiency of the hand. H. T. E. Hertzberg, Anthropometric Unit, Aero Medical Laboratory, Wright Air Development Center.

This paper describes a method of determining the maximum mechanical efficiency of the hand in gripping a dynamometer. A Smedley-type dynamometer is arranged so the distance between the gripping surfaces can be set at 1½″, 2½″, 4″, and 5″ apart. Gripping strengths for 48 subjects were recorded, first with bare hands, then with gloves. It was found that all subjects could exert their greatest force at the 2½″ distance whether barehanded or gloved. The next best distance was 4″. Gloves decreased overall gripping strength by 23.7%. These results provide design data for all manner of levers fitted with hand-released locking devices.

6. *Some vital statistics based on skeletal material.* M. S. Goldstein, U. S. Public Health Service.

(No abstract submitted.)

7. *Genetic influences in body form.* W. W. Howells, University of Wisconsin.

Correlations between pairs of brothers in 20 measurements have already been reported: these showed a high degree of variation between different measurements. Following a factor analysis of the measurements which produced 7 factors, factor scores were obtained for the brothers, who were then correlated in factors as well as in measurements. These correlations produced a definite pattern: in general body size, head size, brain size, lateral cranio-facial growth and ear size, the correlations were close to .38; in long bone length and in facial length the correlations were approximately .62. Because of the higher inter-brother variation in the first group, it is suggested that ordinary genetic factors are more influential in these five factors, and that some further influence, possibly environmental, operates on long bone length and face length. In any case, it is evident that these factor measurements or scores give greater promise for such studies as biometrical genetics, etc., than do ordinary anthropometric measurements.

8. *Biometrics of crowding and spacing of the teeth in the mandible.* C. F. A. Moorrees and R. B. Reed, Forsyth Dental Infirmary for Children, Boston Massachusetts.

Spacing or crowding depend on the ratio between the sum of mesio-distal tooth diameters and the size of the dental arch. These two factors, measured on plaster casts of the dentition of the mandible of 72 American females with a normal complement of teeth, have been subjected to statistical analysis to determine their relationship to crowding and spacing. The sum of the mesio-distal tooth diameters ($M_1 — M_1$) was obtained by adding individual measurements of the teeth, taken at the greatest mesio-distal width at the contact surfaces. The size of the dental arch was secured in two ways in order to approximate the true arch size, namely: (1) the difference between the sum of tooth diameters and the amount of spacing or crowding, if present, (2) the distance from infradentale to the distal contact surface of M_1, which is dependent on length and breadth of the dental arch. The coefficient of correlation between these two measurements is high ($r = .97 \pm .0038$) and they have been used independently in order to eliminate the disadvantages inherent in both.

Crowding or spacing was found to be primarily associated with a lack of correlation between the sizes of teeth and dental arch (99%) and only to a very small degree (1%) with a discrepancy in the range of variation of teeth ($\sigma = 3.59$) compared to the range of variation of dental arches ($\sigma = 3.46$).

9. Basic data on emergence of deciduous teeth in Macaca mulatta. V. O. Hurme, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

Detailed data on the emergence of deciduous teeth in normal *Macaca mulatta*, reared in captivity, have been collected by Prof. G. van Wagenen at Yale University. For all teeth except the second molars the observations were made daily.

Data on 43 to 53 tooth pairs in females and 32 to 45 tooth pairs in males have been analyzed. The following statistics are now available for each class of deciduous teeth: mean, median and modal age of emergence; standard deviation of mean; standard error of mean, based on both number of teeth and on number of tooth pairs; sex difference between means; critical ratio of sex difference.

The material has also been described graphically. The graphs show the most common order of emergence of various teeth, the approximate limits of ranges of variation and the prevalent skewness of the frequency distributions, in addition to the measures shown in tabulations. The dual time scales of the graphs make it possible to read the age either in terms of days after birth, or in terms of decimals of a year.

Additional graphs deal with characteristic patterns of emergence exhibited by animals representing rapid, average and slow dental maturation.

10. Surface texture and histologic structure of chimpanzee teeth; procedures and preliminary observations. E. L. Schuman and R. F. Sognnaes, Department of Anthropology and School of Dental Medicine, Harvard University, Boston, Mass.

In a series of studies on the microstructure of primate teeth it appeared of interest to examine the tooth structure of the ape, combining a collodion replica technique (Pedersen, Am. J. Phys. Anthrop., *8:* 267, 1950) and a histological sectioning method (Sognnaes, Am. J. Phys. Anthrop., *8:* 269, 1950). The material now being investigated originates from a collection of 264 chimpanzee skulls procured from Liberia by Dr. George Harley and placed at our disposal by Dr. Earnest A. Hooton of Harvard's Peabody Museum.

For permanent record purposes individual teeth were first photographed, x-rayed and duplicated in plaster casts. Next, the surface texture was examined microscopically in shadowed collodion replicas. By this method surface irregularities, such as lamination of the enamel perikymata, have been observed. This condition, according to Pedersen and Scott (Acta Odontol., *9:* 262, 1951), is more common in Eskimos than in American whites, and probably of nutritional rather than genetic origin.

Histological sections of the chimpanzee teeth revealed the presence of accentuated enamel striations and interglobular dentin spaces. The latter condition is extremely rare in monkey teeth examined by one of us (R.F.S.), but very common in human teeth, where their presence has been attributed to failures in the calcifying properties of the diet during tooth development.

Finally, it is of interest to note that some of the chimpanzee teeth exhibited lesions which histologically presented the characteristics of dental caries.

11. *The reconstruction of the head of a male Sinanthropus.* G. K. Neumann, Indiana University.

Because of the availability of a greater number of facial fragments of female Sinanthropus skulls Weidenreich reconstructed a skull and head of a female of this form. Most reconstructions of fossil man, however, represent males, because they exhibit specific and varietal differences to a more exaggerated degree. In order to facilitate comparisons the writer modelled a young male Sinanthropus. Although Weidenreich's reconstructed skull formed the foundation for this, measurements and observations on several brain cases and mandibles of males were used to make corrections for the somewhat larger and more muscular male. In addition tissue thickness measurements taken on a male chimpanzee and two robust male Chinese served as a guide to establish maximum and minimum limits. Hair was omitted in order to show cranial contours more clearly. In the reconstruction of the soft parts, such as the nose, mouth, and ears, the writer was guided by modelling them in a generalized yet distinctly human form. The form of the nasal bridge is given by the underlying nasal skeleton, and this in turn gives some indications of the fleshy parts. In this case a primitive medium wide nose, such as that found in certain ''Australoid'' elements of Malaysia, was selected. Its breadth corresponds roughly to the width of the piriform aperture. The mouth is represented as large in conformity with the large alveolar arch; the lips are shown to be of medium thickness; and the ears as unspecialized in form.

SYMPOSIUM ON THE BACK

Organized by Dr. E. Carl Sensenig

1. *The development of nomenclature of the vertebral column.* William S. Cornwell, Eastman Kodak Company, and Department of Radiology, Univerity of Rochester School of Medicine and Dentistry, Rochester, New York.

This introductory contribution is presented to indicate something of the basic thinking in terms of nomenclature that has gone on through the centuries about the vertebral column. Because the anatomy is involved, many terms have been devised to designate the various vertebral parts. Since time permits analysis of only a few terms, the following five have been selected: vertebra, vertebral column, thoracic vertebrae, sacrum, and coccyx.

Though the approach in this study is by way of the meaning of words, hence linguistic, the interest is the history of anatomic concepts concerning the vertebral column. To set the stage, three essential elements are first briefly reviewed. These are the sources of information employed, the historical background of anatomic knowledge, and the current status of vertebral nomenclature.

In discussing the five terms, emphasis is placed on the concepts implied in the Arabic, Greek, Medieval Latin, formal Latin, and vernacular terminologies preceding those in present-day use. Earlier Assyrian and Egyptian terms are mentioned when known. The word lists are presented visually on lantern slides.

2. *Normal embryology of the spine.* E. Carl Sensenig, Department of Anatomy, University of Alabama, Schools of Medicine and Dentistry.

The early development of the human spine has been studied in a series of human embryos. The vertebral elements are derived from sclerotomic tissue which migrates from the paraxial or somitic mesoderm. This tissue forms a medial concentration about the notochord and a lateral concentration, continuous with the former, whose dorsal extensions form the vertebral arches and whose ventrolateral extensions form the proximal part of the rib shaft. These concentrations, except in the rib rudiment, are rostrocaudally continuous and segmentation is only indicated by the intersegmental vessels. Medial concentrations are divided into cranial and caudal sclerotome-halves by a sclerotomic fissure. A dense celled aggregation, which occupies the posterior one-third of the cranial and the anterior two-thirds of the caudal sclerotome-halves, forms the perichordal disc. A loose celled aggregation, occupying the posterior one-third of the caudal and the anterior two-thirds of the cranial sclerotome-halves, forms the primary centrum. The perichordal disc becomes transversely subdivided into three areas, of which the intermediate area gives rise to the intervertebral disc. The definitive centrum is formed by the incorporation of the adjacent parts of the perichordal discs onto the rostral and caudal ends of the primary centrum. A definitive vertebral segment is formed by the rearrangement of sclerotome-halves in which the caudal half of one segment combines with the cranial half of the next following segment. In the definitive column the intersegmental vessels lie at the midpoint of the vertebrae, while the sclerotomic fissure is represented by the caudal centra-intervertebral disc articulation.

3. *Factors influencing the development of the vertebral column.* Howard Holtzer,[1] Department of Anatomy, College of Physicians and Surgeons, Columbia University.

The role of the spinal cord in modeling the vertebral column was evidenced by the following: (1) Partial extirpation of the neural tube in Amblystoma embryos resulted in spinal cords whose diameters ranged from 75% to 10% of the controls. In each case the diameter of the spinal canal underwent a reduction proportional to the reduced dimensions of the cord. (2) Operations producing enlargement of the spinal cord up to 200% of controls eventuated in correspondingly enlarged neural arches. (3) Complete extirpation of a 4–6 segment length of spinal cord resulted in total absence of vertebrae. Instead, a continuous solid cartilaginous rod, independent of the notochord, extended the length of the region lacking spinal cord. The notochord in this series underwent precocious and atypical cartilaginization. (4) Supernumerary neural tubes, grafted dorsad to the host's cord, yielded two rows of neural arches, one dorsad to the other. (5) Displacement of the spinal cord dorsad, with respect to the notochord, was followed by cartilage formation around former, leaving the notochord bare. (6) Notochordectomy did not inhibit formation of vertebrae. (7) Rib-bearing process and ribs failed to develop when neural arches were suppressed. (8) Unless precartilage cells migrate from somites they will not form cartilage.

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[1] U. S. Public Health Postdoctorate Fellow.

4. *Scope and limitations of genetics and the regional growth concept for interpretation of variations in the back.* P. B. Sawin, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine.

Gene induced variations transmitted as simple Mendelian recessives (or dominants) in laboratory and domesticated animals and man are found at either extremity (tail or skull) of the axial skeleton and are frequently lethal. Variations more centrally located appear to occur with equal or greater frequency, possess greater survival value and are transmitted in a less clearly defined manner. Dominance is lacking, and blending inheritance, characteristic of polymeric genes, is typical in the first hybrid and subsequent generations. Penetrance and expressivity are such as to indicate a substantial effect of minor intra-uterine or other environmental influences during development. Individual genes appear of minimal value in interpretation, but collectively such variations as extra ribs, extra presacral vertebrae, number and size of sternebrae, ventral spinous processes, change in position and function of transverse, zygomatic and other processes, time and order of appearance of primary and secondary centers of ossification and of regional epiphyseal union, all seem to contribute to a definition of limits and differences in underlying regional growth processes and patterns comparable if not homologous in many ways to the growth gradients of the embryologist. Evidence is available, however, to show that similar influences are effective postnatally, sometimes obliterating the variation induced prenatally. Nor are variations so induced confined to the skeleton, but pattern of vessels arising from the aorta, size and shape of gall bladder, size and lobation of the liver, and size and shape of glandular organs are also involved. Because of the profound influence upon growth, reproduction and behavior, the last named may have particular implications in connection with abnormalities associated with hormone imbalance, abnormal growth and degenerative changes of later life and with irregularities of reproduction.

5. *The evolution of the vertebral column.* Frederick P. Thieme, Department of Anthropology, University of Michigan.

A review of the evolutionary development of the vertebral column from the prechordates to man. The phylogenetic history as seen in the light of adaptive locomotive system which made the human upright posture possible and at the same time accounts for much of the morphology of man's spine. This is done in terms of the basic features developed through evolutionary time and in terms of a modern theory of evolutionary process. Particular emphasis is given to the ''heritage'' versus ''habitus'' characteristics (Gregory) as a methodology for the interpretation of morphological variability. In this respect, function, as the most important ''habitus'' influence, is seen to be the mechanism through which natural selection operates. To a greater or lesser extent, analysis of variability in relation to age, sex, occupation, nutritional background, and other isolable factors arising from the individual's ecological position and history will give estimates of the role of function in determining morphology. In addition, analysis of the whole muscle-bone-posture complex may allow a detailed mechanical explanation of the process of morphological determination resulting from functional demands. Analysis of evolutionary patterns, racial variation, and detailed genetic analysis in samples of known relationship can help to give an understanding of ''heritage'' morphology. It is suggested that complete understanding of the human vertebral column must depend upon the summation of results from many avenues of investigation.

6. *Some critical stages in the evolution of the human back.* William K. Gregory.

The fossil record, dating from the Upper Ordovician some 400 million years ago, begins with primitive piscine chordates, the ostracoderms, which were metameric and bilaterally symmetrical, differentiated both cephalocaudally and dorsoventrally, provided with a notochord as an elastic axis, and moving forward by rhythmic undulatory thrusts of the body walls against the water. No calcified centra, neural arches, zygapophyses, transverse processes or ribs were present. The head and anterior part of the thorax were roofed with exoskeletal plates or scales, sometimes with bone cells, and relatively inflexible — the fulcrum for the forward thrust of the metameres; median and pectoral fins of relatively simple construction as minor accessories to the lateral myomeres.

For the second major stage (omitting intergrading conditions) we may choose *Eusthenopteron,* a Devonian choanate fish, with well developed paired and unpaired fins, pectoral girdle attached above to corners of skull, pelvic rods confined to ventral surface, no sacroiliac junction, well developed neural arches, and segmental centra of anterior vertebrae strengthened to transmit thrusts from column to skull.

In the third stage, represented by the air breathing tetrapods of Mississippian and Pennsylvanian times, the paired limbs propelled the body and formed with the vertebral column ''the bridge that walks.'' The shoulder girdle became loose from the skull and formed with the sternum and parasternum a wide supporting crib. The sacroiliac attachment, at first loose, tended to divide the column into pre- and post-sacral moieties, while the freeing of the shoulder girdle from the neck encouraged differentiation of the cervical from the thoracic regions.

In the fourth stage, represented by the cynodont reptiles of the Triassic, it may be inferred that at least an incipient diaphragm was evolving, for the differentiation of the thoracic and lumbar regions was becoming conspicuous. The extended lumbo-sacral-coccygeal joints allowed only slight dorsoventral but somewhat larger lateral flexure and torsion.

In the fifth stage, represented by the Eocene lemuroid *Notharctus,* the limbs and column were adapted for the free locomotion of arboreal life. Lumbar parapophyses indicate sharp flexure and extension in the vertical plane; the iliosacral contact was short but supplemented by widened coccygeal transverse processes. The tail, with feeble locomotor power, was beginning to form the muscular floor of the pelvis.

In the sixth and seventh stages, free leaping and partly brachiating was gradually lost as tree-living gave place to secondarily ground-living bipedal stance and locomotion. As erect posture increased, the column sagged or buckled into compensatory cervical, thoracic and lumbar curves; the sacrum widened greatly with the increase of the erectors of the spine; the ilium retained more of its foetal and infantile shortness; the lumbar region may have been secondarily lengthened and its forward curve increased almost or quite to the danger point.

As thus conceived, the evolution of the back was not orthogenetic but revolutionary, in adjustment to the successive shifts from water to land, up into the trees and down again upon the ground.

7. *Some considerations of "normal" scoliosis.* T. D. Stewart, U. S. National Museum, Washington, D. C.

Idiopathic scoliosis presents some rather constant characteristics that have led a number of investigators to look for predisposing factors in the bones of the spine. It is noteworthy, for example, that severe scoliosis develops during childhood and especially around adolescence; that about 80% of the cases are female, and that the curvature is predominantly convex to the left in the lumbar region and convex to the right in the thoracic region.

In contrast to severe scoliosis, so-called "normal" scoliosis amounts to little more than asymmetry. The object of this paper is to examine the asymmetries of normal spines to see whether they parallel to any extent those in the serious type of scoliosis, and therefore might constitute incipient or predisposing stages. Special emphasis is placed on asymmetries of the vertebral centra.

For the present purpose two series of spines have been examined, one of Eskimos and the other of Pueblo Indians. In both groups it is usual to find some or all of the centra between T4 and L2 flattened on the right side. Probably this flattening has been produced by pressure of the aorta on the left.

The thoracolumbar transition vertebrae also frequently present asymmetries of the facets and costal elements. All such features make for instability. Yet the fact that they can occur without gross curvature developing, would indicate that the inciting factor in idiopathic scoliosis comes from outside the bones.

8. *Incidence of separate neural arch.* Maurice B. Roche, and George G. Rowe, Departments of Surgery and Anatomy, Washington University, School of Medicine.

A detailed survey of the lumbosacral columns of 4,200 skeletons was made in order to determine the incidence of the separate neural arch as well as the coincidental appearance of certain specific congenital or acquired characters. The study was confined largely to the lumbar vertebrae and was extended to the rest of the column only when a lumbar vertebra was shown to present a separation through the pars interarticularis of the neural arch. All columns presenting isthmal defects were subjected to additional examination for the following characters: total number of presacral vertebrae, articular facets for cervical ribs on the seventh cervical vertebra, spina bifida, articulations of the transverse processes of the fifth lumbar vertebra with the sacrum or ilium or both, arthritis of the central atlantoepistrophic joint, arthritis of the articular processes of the lumbar vertebrae and certain anatomical variations in the sacrum, including accessory sacroiliac articulations, incomplete fusion of the first sacral segment, spina bifida of the upper sacral segments, complete spina bifida or open dorsal bony wall of the sacral canal, and rudimentary sacral articular processes.

DEMONSTRATIONS

A. *The definition of thoracic and lumbar vertebrae.* S. L. Washburn and J. Buettner-Janusch, Department of Anthropology, University of Chicago.

Traditionally thoracic vertebrae are defined as those which bear ribs. Lumbar vertebrae are those which lie between the thoracic vertebrae and the sacrum. An alternative method for dividing the vertebrae is by the type of facet. The lumbar-type articulations limit motion to flexion and extension, and the presence of lumbar facets correlates with arrangements of muscles, motions and much vertebral anatomy. In man and apes division by counting ribs or by analyzing by facet-muscle-motion gives the same results, but in monkeys the vertebrae divide differently by the two methods. There are two more lumbar vertebrae by the facet-counting method, and there is no overlapping between the apes and monkeys. The functions of the vertebral column are different in the quadrupeds and in the brachiators and bipeds. Different functions are reflected in the deep anatomy of the vertebrae but not in the number of ribs.

B. Axial skeleton of an Upper Palaeolithic woman from Hotu. J. Lawrence Angel and Carleton S. Coon, The Daniel Baugh Institute of Anatomy, Jefferson Medical College and the University Museum of the University of Pennsylvania.

A complete vertebral column and pelvis of Hotu no. 2, carefully restored and mounted with disks of average American thicknesses (Todd and Pyle, '28) plus careful adjustment of facets, is almost unique for this early date. The mounted column measures ca. 71 cm along the curves and 62 cm directly atlas-6th sacral (57 and 54 cm respectively for presacrals). Atlas-ischium distance is almost 70 cm, suggesting sitting height of over 87 cm compared to stature reconstruction of over 167 (long bones Dupertuis formula). Relatively horizontal sacrum (33 degrees), tilted pelvic brim (68), and sharp lumbo-sacral angle (39) go with emphatic presacral curves (lumbar index 95.7?) especially at L4 – S1 and Th 10–12. This fits indications of muscular strength (cf. Goff, '51). Medium to marked bony lipping replaces stressed fibres of the L5 – S1 disk ventrally and small pathological erosions in facing surfaces of L5 and S1 indicate herniating Schmorl bodies. Slight lipping of vertebral body rims occurs from thoraco-lumbar transition up to about Th 5.

The pelvis, deep (inlet index 102) and narrow (bi-iliac 256) with capacious outlet (intertuberous 125) shows pronounced ligament and muscle attachments throughout, though pubic symphysis is early Phase V. This helps to support the inference that Hotu no. 2 was a strong woman living under much physical stress, probably including one or more pregnancies: there are no signs of disease other than bony response to strain on fibrous tissue, though a huge dental cyst and traces of osteoporosis occur in the skull.

C. The ossification of the axial skeleton in man. Charles R. Noback and G. Gordon Robertson, Departments of Anatomy, Columbia University and Baylor University.

Each bone of the axial skeleton ossifies from more than one ossification center. The manubrium is an exception. The primary centers of each of the bones of the vertebral column, of the sternum and of the ribs appear before birth. The primary centers of the coccyx and xiphoid process are exceptions. They appear after birth. The secondary centers appear after birth.

Primary centers: The centers of the vertebral centra and neural arches appear concurrently. The cervical and upper two thoracic arch centers appear almost simultaneously but not in a definite order. The other neural arch centers appear subsequently in a cephalo-caudal sequence. The centra centers — one for each centrum — appear initially in the lower thoracic and upper lumbar region; the others appear cephalically and caudally from this location. The first ribs to ossify are the 6th and 7th ribs; from this site the other costal centers appear cephalically and caudally. Seventh cervical costal cartilages are frequently ossified — present in 34 of 79 fetuses from 57 to 235 mm CR length. Of the 34 fetuses, 24 fetuses had paired centers. The sternal centers appear during late fetal life in a cephalo-caudal order. The primary centers of the vertebral column fuse during the first decade of life. The arches fuse in the midline in the regional sequence: lumbar, thoracic, cervical and sacral regions. The arch and centra fuse in a cephalo-caudal sequence.

Secondary centers: The secondary centers of the axial skeleton appear during puberty and fuse during the third decade.

D. Congenital anomalies of the atlanto-occipital regions. Lee Hadley, University of Syracuse, School of Medicine.

(No abstract submitted.)

E. Separate neural arch. Maurice B. Roche and George G. Rowe, Departments of Surgery and Anatomy, Washington University, St. Louis.

Characteristic specimens will be presented from 32 lumbosacral columns previously exhibited as part of a survey of 4,200 skeletons bearing on the incidence of the separate neural arch. The specimens to be presented are lumbosacral columns showing unilateral and bilateral neural arch defects, variation in the level of occurrence, multiple clefts in one neural arch, defects at multiple levels in one column, associated lumbosacral variations and arthritic manifestations.

F. The early development of a vertebra as demonstrated by photo-transparent models. E. C. Sensenig, C. F. Reather, O. O. Heard and R. D. Grill, University of Alabama, Schools of Medicine and Dentistry, and Carnegie Institution of Washington.

The early development of the human vertebra is demonstrated through the use of photo-transparent models. The demonstration consists of a photo-transparent model of the same thoracic segment in 9.0, 13.0, 18.0, 31.5, 41.0, 54.0 and 80.0 mm human embryos. In each case every section in the transverse series of the segment between two adjacent intersegmental vessels is photographed. These photographs are printed on film and tinted with color (Kodak Dye Transfer Process). The dye intensity is equalized on each print. The prints are then stacked in sequence and viewed through an x-ray reader. In this manner a three dimensional picture of the developing vertebral elements is obtained. The present series demonstrates the early vertebral mesenchymal concentrations and follows the development of these concentrations through the membranous, precartilage, cartilage and initial ossification stages.

G. Normal scoliosis. T. Dale Stewart, U. S. National Museum.

(No abstract submitted.)

H. Use of a photoelectric reflectometer in the measurement of human skin color. Gabriel Lasker, Wayne University College of Medicine.

A photoelectric reflectometer has been applied to determinations of skin and hair color in Lee R. Dice's laboratory at the Institute of Human Biology at the University of Michigan and Neil Tappen demonstrated the apparatus to us. Now we also use a ''Photovolt model 610'' for securing measurements of diffuse reflectance of various colors of light from human skin. The search unit of the apparatus can be applied directly to the skin. It consists of a photoelectric cell similar to that of an ordinary light meter, but with a built in light source and a slot for filters. Recordings are made on a linear scale from black as 0 to a white enamel standard as 100. Degree of darkness of various areas of human skin can be differentiated in objective terms. Quality of skin color can be determined by comparing readings with red, green and blue filters. This apparatus fills a need for readily repeatable stable measurements of skin color. It can be operated on a storage battery and is portable and sturdy.

9. Pathology of spondylosis deformans. Golden Selin, Department of Pathology, Hospital for Joint Diseases, New York City.

In this study, special attention was directed to lesions of spondylosis deformans in the early stages of their development. Vertebral columns were removed at autopsy, cleaned, photographed, and x-rayed intact. They were then sectioned in a number of different planes, and the slices, in turn, were photographed and roentgenographed. In taking the tissue blocks from the slices for histologic study, we were guided by the roentgenographic appearance of the various areas. This was done in cognizance of the fact that slight deviations from the normal, with which we were particularly concerned, were readily seen in the roentgenographs, but were as yet not observable on gross examination.

We have concluded that a very early, if not the first, demonstrable local alteration leading to the development of spondylosis deformans lesions occurs in relation to the marginal ridges of the vertebral bodies, at one or more sites where the body is covered by anterior ligament. At these sites, one notes a slight ''humping up'' and outward exostotic projection of the ridge. This comes to be associated with a slight forward projection of the disk and adherent ligament. As the alterations progress, one notes the development of ledges or buttresses of variable size at the ridge-cortex junction. The buttresses tend to grow toward each other, sometimes even fusing, and compress between them, or even pinch off, the drawn-out disk tissue.

Laterally — that is, beyond the region of the ligament — the bony ledges are likely to be particularly striking, since the absence of the ligament removes a restraint upon their growth. When a high degree of spondylosis deformans has developed between two vertebral bodies, some slight ledge formation at the marginal ridge may also occur posteriorly, in relation to the vertebral canal, but striking ledge or buttress formation does not take place.

10. Motion picture studies of the cervical spine. George H. Ramsey, Department of Radiology, University of Rochester. (Introduced by E. Carl Sensenig.)

A new equipment and technique is available for the study of many areas of motion of the body. A brief history and description of the equipment will be given and a motion picture shown. These films are a few from the study now under way and is shown to inform you of the method rather than to exhibit any definite results. The radiologist is interested in the normal mobility and stability of the cervical spine in the various decades of life and in the changes found in disease and trauma.

It is hoped from the study of these films to arrive at findings that will add to the knowledge of motion of the cervical spine in health and disease and will allow more certain analysis of static radiographs as commonly available and used.

11. Round back. Beckett Howorth, Department of Orthopedic Surgery, College of Physicians and Surgeons, Columbia University. (Illustrated with standard 3¼ × 4¼ lantern slides.)

Human posture and its evolution. Development of posture in the infant and child.

Round back: definition and types. The round back of infancy. Adolescent round back, or Scheuermann's disease. Senile round back.

Relation of round back to lordosis and to scoliosis.

Etiology of round back.

Pathology of round back.

The effect of round back on the subject: posture, symptoms, physical signs. Laboratory findings.

The roentgenographic picture of round back.

Illustrative cases.

What are the zoological relationships and implications of round back?

12. Clinical aspects of the low back. Charles W. Goff, Department of Orthopedic Surgery, Yale University School of Medicine, Hartford, Connecticut. (Time: 15–18 minutes. Slides [12]: 3¼ × 4¼.)

Contributions from parallel disciplines have been reluctantly received by the medical clinician because of an inherent lack of breadth of education of the physician. Only a few outstanding medical schools have instructional courses devoted to anthropological subjects.

Clinically the correlations between fields of interests are obvious. The Army series of body build studies, Thieme's syllabus on the lumbar breakdown, Stewart's osteologic studies, Trotter's reports, Krogman and Reynolds' growth studies are a few that come to mind.

Summaries have been published of large series of industrial back disabilities. The latter offer important evidence of our greater emotional rather than scientific approach to the region under discussion. In these clinical studies a few knowns are sorted again and again while the many unknowns are made the basis of irrational judgments, often repeated until they are assumed to exist as scientific facts.

A determination of the cause of low back pain is difficult. Many causes are classified and a few are discussed.

Techniques of diagnosis are many. After they have been applied, judgment factors, plus the emotional thinking of the physician, must be brought to bear in establishing likely conclusions. A recent syllabus on low back pain begins with the statement that ''an accurate and exact diagnosis'' must be made. Very soon thereafter the expressions used change to ''presumed pathology.''

Diagnostic tests are discussed from the clinical viewpoint and treatment mentioned in outline. The emotional overlay, existing in most all sufferers from low back pain, is discussed. Methods of determining the malingerers are mentioned.

REGULAR PROGRAM (continued)

12. A comparison of split-line patterns in man and chimpanzee. Neil C. Tappen, Institute of Human Biology, University of Michigan. (Slides: Two 2″ × 2″.)

Benninghof split-line preparations on human and chimpanzee skulls show some major differences in the pattern of the face region. These differences appear to be related to some of the differences in structure of different parts of the face (nose, orbits, brow ridges). Further differences are associated with different anatomical relationships of the masseter muscle to the face. By comparing different species, it is strongly indicated that there is a relationship between gross structure of the bone and its minute architecture in the compacta. As an example of the kind of applications to anthropological problems made possible by such comparisons, factors that seem to enter into the formation of the canine fossa in man are discussed.

13. Facial suture closure in whites and American Negroes. W. Montague Cobb, Department of Anatomy, Howard University.

Previous investigations of suture closure have dealt chiefly with the sutures of the cranial vault, with only cursory reference to those of the facial skeleton. The present report states the findings on 2351 adult skulls between stated ages of 21 and 96 years in the documented skeletal collections assembled by T. Wingate Todd at Western Reserve University and Robert J. Terry at Washington University. The reported series includes 1589 males (808 Negro and 781 White) and 762 females (474 Negro and 288 White). The 13 sutures included are: intermaxillary, interpalatal, maxillopalatal, intermaxillary (buccal), internasal, nasomaxillary, maxillomalar, frontomalar, temporomalar, sphenomalar, frontomaxillary, frontonasal and pterygopalatine. Tables present the distribution of the skulls and the incidence and degree of closure by sex, race and age.

14. Structural alterations in rats hypophysectomized at six days of age, and their prevention or repair by growth hormone.[1] Donald G. Walker,[2] C. Willet Asling, Miriam E. Simpson and Herbert M. Evans, Division of Anatomy and Institute of Experimental Biology, University of California.

Sixty-one rats were successfully hypophysectomized when 6 days old. Forty-three received no replacement therapy, while 18 were treated with growth hormone from the 30th to the 80th day of age or longer. The untreated rats differed from rats hypophysectomized when 28 days old in several critical respects. They invariably died before 80 days of age. Deaths were preceded by behavior related to neural damage. At autopsy their brains were of normal size, though their crania were dwarfed, and compression and herniation of the brain were demonstrable. Though the entire skull was markedly stunted in length, the brain case was wider than normal. When treated with growth hormone such rats survived, showing no evidence of neural damage. They grew vigorously, and came to possess crania as large or larger than normal. The non-endocrine viscera grew in proportion to body weight, but some of them (especially the kidney) did not undergo normal differentiation, nor did skeletal differentiation occur. The endocrine viscera remained atrophic.

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[1] Supported by a grant from the United States Public Health Service (GG-409).

[2] Present address: Department of Anatomy, University of Oregon.

15. A biometric study of human mandibular segments. Melvin L. Moss, Department of Anatomy, Columbia University. (Introduced by Charles R. Noback.)

A method of measurement of the segments of the human mandible is described, together with a statistical study of the variation exhibited. The isolation of the segments and their essential asymmetry is demonstrated, with an evaluation of the factors producing overall bilateral symmetry.

Four hundred and forty-four mandibles are measured. Three segments of the ''neutral'' portion of the mandible are located: from the caput of the condyle to the lingula of the mandibular foramen, from the latter to the mental foramen, and from the anterior borders of both mental foramina. The constant asymmetry of each of these segments, the dextral dominance, and the failure of the mentongonion measurement to demonstrate these is statistically analysed.

The effects of endochondral and appositional bone growth are reflected to varying degrees in each of these segments. This method of mensuration gives a technic for ascertaining the quantitative aspects of each of these modes of bone growth and of the interaction between them.

Some further applications of this technic for relative growth study and other physical anthropological inquiries are mentioned.

16. Newer methods for studying the functional significance of bone form.[1] F. Gaynor Evans, Department of Anatomy, Wayne University.

Ever since J. Wolff proposed his theory of bone transformation and Culman compared the femur to a Fairbairn crane, biologists have been interested in the functional significance of bone form. This fundamental problem was formerly studied with reference to the trabecular orientation of spongy bone and by elaborate mathematical analysis of bone sections or intact bones under assumed conditions of loading.

In the last two decades, however, there has been an increasingly greater use of techniques employed by engineers for studying similar problems in various engineering or mechanical structures. One of the most popular has been the photoelastic method in which the stresses and strains produced in a plastic model of a bone subjected to varying conditions of loading are studied under polarized light.

Other techniques in which actual intact bones are used instead of models include the colophonium and ''stresscoat'' methods as well as the use of extensometers and electric strain gauges attached to the bone. All of these methods have been employed in studying the deformations and stress-strain relationships in the skull and long bones. These phenomena have been studied in the bones of living animals as well as in individual intact cadaver bones.

Each of the aforementioned methods was illustrated and their relative merits discussed.

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[1] This investigation was supported (in part) by a research grant from the National Institutes of Health, United States Public Health Service.

17. *An approach to the localizing of pivotal axes of the major extremity joints.* Wilfrid T. Dempster and George Suzuki, Department of Anatomy, University of Michigan.

Conventional measurements based upon standard anthropometric points have value because calipers placed upon arbitrary palpable landmarks give readings that can be duplicated. Landmarks, however, have no significance beyond accessibility, and the measurements imply no more than relative magnitude of the body regions. In spite of mathematical treatments of data, measurements still deal only with arbitrary body dimensions.

We are here attempting to locate functionally significant axes of movement in the major extremity joints and fit them into the linear pattern of surface landmarks of the whole extremity as measured in anthropometry. Pivotal axes of movement may be correlated with the radius of curvature of joint surfaces, and this latter measurement has been explored in our work. Our sample consisted of both cartilage covered upper and lower extremity bones from 20 male cadavers and a parallel series of random dry bones. Radius of curvature was determined geometrically from dial gage measurements of the depth of arc which corresponds with the convexity or concavity of the joint surfaces. For the humerus and femur, the interpivotal distance between proximal and distal joints equals the bone length minus the radii of curvature at the two ends. The interpivotal length for the forearm and leg included the interarticular length of the segment plus the humeral or femoral condylar radius of curvature plus, also, the carpal or talic radius of curvature. For each extremity, radii of curvature, interpivotal distances and actual axes of movement will be shown in relation to the linear pattern of anthropometric points.

18. *The geographic distribution of heterozygotes for essential pentosuria.* Margaret Lasker, Yonkers, New York.

The application of a simple chemical test for pentose (xyloketose) in urine, designed for clinical use to distinguish essential pentosuria from diabetes, has also made it possible to assemble data of genetic and anthropological interest.

The outstanding fact which emerged from the discovery of a series of cases of this recessive anomaly was its almost exclusive occurrence in Jews. In order to explain this phenomenon, an effort was made to locate a possible single mutation from which the different cases might have arisen. The result of search into family histories, however, showed that not even two families were connected by blood within living memory. If a single mutation did occur, it must have taken place at a very much earlier date.

A map of Europe showing the location of genes in the grandparents' or parents' generation of 57 families with pentosuria cases indicates a rather wide distribution — from Riga on the Baltic to Odessa on the Black Sea, from Holland to Hungary. This corresponds to the region of largest Jewish settlement towards the end of the nineteenth century. There is, however, one area of this region where the concentration of genes is especially high, namely the southern part of Hessen, where the Main joins the Rhine, while the great majority of genes are found in northern Poland and the adjoining Russian provinces.

An estimate based on all cases known to the writer to be firmly established shows that the probable incidence of the gene for pentosuria in the Jewish population is between one in 40 and one in 60. This being so, there exist approximately 100,000 genes in the five million or so Jews of this country. On the other hand, in the non-Jewish population not even one gene occurs in 800 persons.

A calculation based on the hypothesis of a single mutation (the hypothesis which seems the most probable in view of the gene distribution) and on the total gene incidence, above, dates the mutation back about one thousand to two thousand years. However, factors such as different amounts of inbreeding, adaptive advantages or disadvantages, of which none are known at the present time, or a change in the average size of Jewish families would change the results of this calculation.

19. *The sickle cell trait in Africa.* Ronald Singer, Anatomy Department, University of Cape Town, and Department of Embryology, Carnegie Institution of Washington, Baltimore. (Introduced by Gabriel Lasker.)

The anthropologist's main interest in the blood of the African Negro groups lies in its genetical and ethnological significance. If the distribution of the trait has an ethnographic basis, then there must be a re-evaluation of Seligman's hypothesis. The sickling condition is apparently found exclusively in Negroes or in people who have had Negro admixture. Available evidence is discussed. The grave possibility of fallacies in previous reports and data leading to incorrect theories is outlined. The distribution of the trait suggests that it arose somewhere near the Ruwenzori Mountains between Lakes Albert and Edward in Central Africa. From there it spread with the migrant ''Negro'' over the vast continent thinning out the farther it emanated from its source. In Cape Town the incidence in 1,555 Cape Coloureds — a physically homogeneous ''hybrid'' race whose root elements were the Hottentots with admixture from Bushmen, Europeans, Asiatics, and Bantu — was found to be 0.58%, in comparison with 45% in the Baamba, a segregated pygmoid people living to the west of the Ruwenzori.

The incidence and distribution of the trait produce a number of highly important problems which are discussed.

Africa is an untapped blood-pool, and only more detailed and accurate data may provide the answers. The distribution of the blood groups may give useful correlations. African Negro groups are unique in possessing an enormously high percentage of the genotype Rh_0 or cDe. Whether this applies equally to the true Pygmy Negritoes, Hamites, Bushmen, and Hottentots is yet to be discovered.

20. Research among the Bushmen and Hottentots in South Africa. Martin Gusinde, Department of Anthropology, Catholic University of America, Washington 17, D. C.

In the Southern half of Africa, i.e. South of the Sahara, there are two districts of which the aborigines distinguish themselves very clearly from the genuine Negroes by their light-yellow skin and by a considerable number of other bodily characteristics. These are the Pygmies in tropical Africa and the so-called ''Yellow Peoples'' in the desert areas of South Africa: the Bushmen and Hottentots. Ever since they were discovered their peculiar body-formation has challenged the anthropologists for its correct explanation.

After having accomplished a very detailed anthropological examination of the Pygmies in the Eastern tropical forest of the Belgian Congo and in Ruanda during 1934–35, another expedition, supported by a grant of the Wenner-Gren Foundation (Viking Fund), was undertaken through the desert areas of South Africa. The main objective was to determine the physical characteristics of the Bushmen and Hottentots, in order to find out if there is a genetical relation between these ''Yellow Peoples'' and the real Pygmies.

From the very start it appeared to be necessary to traverse all larger districts where Bushmen and Hottentots are still living their free lives as of old, as primitive hunters and foodgatherers. About 800 grown ups had been thoroughly examined. The result of these long and troublesome trips through the sandy territories of the Bushmen was this: The pure blooded !Kun show forth such typical body marks that close biogenetical relations with the light skinned Pygmies cannot possibly be proved; but influences from a somatic type of the Hamitic peoples must likely be assumed. The bulk of the !Kun inhabits the inner circle of the Kalahari-Desert. In the Okawango Swamps there lives a group of Bushmen called the Kanikwe, who have remained almost completely pure blooded, but have become fishermen. At the western entrance of the Caprivi-Strip there is located the group of the Barakwengo, who even today live like the true and genuine !Kun — the primitive lives of nomads. They have however taken in a considerable amount of Negro-blood. The frequently mentioned Hei!kom South-east of the Etosha-Pan were originally Hottentots and are now, since a number of decades, amalgamated with Bushmen, Berg-Dama and Ovambo-Negroes; they lead however the lives of primitive foodgatherers and number about 300.

The territory of the genuine !Kun extends to the North beyond the 14° and to the South to the 27°. They move from East to West between 14° and 22° Eastern longitude. Their total number is about 10,000, of whom 3,000 live in Angola and 7,000 in the desert areas South of the political boundaries between Angola and South-West Africa.

There is no longer any reasonable doubt that the Bushmen have to be considered an independent race with outspoken typical somatic marks.

21. The Australopithecine sites. G. B. Barbour, University of Cincinnati.

(No abstract.)

22. Equations for estimation of stature from long limb bones for American White males.[1] Mildred Trotter and Goldine C. Gleser, Department of Anatomy, Washington University, St. Louis.

Regression equations for estimation of living stature from lengths of single and multiple long limb bones of 710 American White males are presented. The data on which these equations are based are unique since stature was measured during life and the bones after skeletonization. Another refinement provides an adjustment of the equations to account for the effect of ageing on stature.

A comparison of statures estimated for another sample of American White males according to the equations involving humerus and femur is made with estimates derived by pertinent equations of other investigators. The formulae developed in this study provide estimates of stature which are closer to the recorded statures than do the other formulae. The equation based on femur plus tibia is also applied to this new sample, and an almost exact estimation of the mean stature is obtained with a standard error in close approximation to the expected value. Thus, these equations based on long limb bones of American White males have provided accurate stature estimates for a new sample drawn from the same population.

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[1] This investigation was supported (in part) by the Department of Army through its contract (no. DA44-109-qm-199) for Research, Development and Technical Services with Washington University.

23. Cephalo-facial growth in identical girl twins. W. M. Krogman, University of Pennsylvania and Children's Hospital.

(No abstract.)

24. Individual and group deviations from ''channelwise'' grid progression in girls. Stanley Marion Garn, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

The idea of ''channelwise'' progression, one of the basic features of the Wetzel ''grid,'' has been tested on 97 Boston white girls, including 32 from the Study of Child Health and Development, directed by Dr. Harold C. Stuart, and 65 from the Forsyth Growth Study. In the longitudinal (Harvard) series, the per cent of girls remaining in the ''starting channel'' (channel at age 6) dropped to 50% at age 7, and 19% at age 8. Thereafter, not more than 33% were in the ''starting channel'' at any time up to age 16. In the Forsyth series a shift of one or more channels occurred in 58%, within a period of one year. In both series there was a marked tendency for younger girls to lose channel position, and older girls to gain channel position. The mean and modal paths as plotted on the ''grid'' were trough-shaped, rather than straight, in both series. Mean data on 6 groups of girls selected from the literature traced similar trough-shaped paths when plotted on the ''grid.'' It was concluded that constancy of channel position is not a modal phenomenon in growing girls as noted by both Døssing and Andersen, and that the grid construction does not entirely correct for changes in female body build during growth and maturation.

25. The time of eruption of second permanent (12 year) molar and its relation to body size and sexual maturation. Danaë Apostolides-Talmers, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

It has not yet been determined whether or not dentition is a satisfactory measure of maturation.

In the present study an attempt has been made to clarify the problem, studying the relationship between the eruption of second permanent molar and height, weight and areola development.

A total of 176 children from the Forsyth patient population was divided into 5 small groups:

1. A group of 50 boys early in their dental eruption.
2. A group of 50 girls similarly early in their dental eruption.
3. A group of 20 boys late in their dental eruption.
4. A group of 20 girls also late in their dental eruption.
5. A group of 36 girls advanced as far as areola development is concerned.

For the groups selected on the basis of the eruption of the second permanent molar the overall growth status was assessed by means of height and weight measurements and for the early sexually maturing girls the dental eruption was determined from their dental records.

The results indicate that early eruptors of both sexes tend to be taller and heavier for their age. Conversely, late eruptors are well below average in both height and weight. The girls advanced in sexual maturation are for the most part also advanced in dental eruption.

It is concluded that, although greater samples and relationship with other maturational criteria should be investigated, the findings are highly suggestive of the value of the eruption of the second permanent molar as an aid for determining growth status during adolescence.

26. A review of menarche. H. J. Colby, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

(No abstract.)

27. Comparison of areolar stage and bone age. M. P. Crawford, Forsyth Dental Infirmary for Children, Boston Massachusetts.

(No abstract.)

28. A study of obese children selected on the basis of fat pads. Peggy Crooke Fry, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

Of 414 children included in the Forsyth Child Growth Program, 39 (26 girls and 13 boys) were selected for special investigation on the basis of body fat pads or folds on three body regions as seen in somatotype pictures. Data was gathered and analyzed on height and weight, skeletal maturation, tooth emergence, protein and caloric intake and family background. Analysis of these factors showed that: (1) Height and weight comparisons were markedly divergent, with 95% of the sample exceeding the age norms in weight, while only 59% were comparably above the norms in stature. (2) Sixty-seven per cent of the sample were advanced in bone age, with a range of bone age quotients from 68 to 153. (3) Tooth emergence data showed 75% of the sample as advanced, with standard scores for tooth emergence up to + 1.59. (4) The diets of these children were not loaded in favor of calories or fat-building substances. (5) Familial background including the psychic environment, ethnic origin and a tendency toward fatness in one parent were not found to be contributory factors.

29. The distribution of ABO blood-groups in Afghanistan. George Maranjian, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

Two thousand nine hundred and twenty-one individuals were blood-grouped in Afghanistan. The prinicipal groups, with the corresponding gene frequencies, p, q and r, are:

		p	q	r
Tajiks (Farsi speakers)	1355	.222	.238	.545
Pathans (Pushtu speakers)	835	.225	.244	.536
Hazara (Farsi speakers)	171	.175	.222	.612
Uzbeg (Turki speakers)	74	.329	.175	.493
Eight other groups	486		(Not included)	
	2921			

Afghanistan conforms fairly well to Boyd's isogenes. The gene frequency of A(p) in most of our groups, falls between 0.200 and 0.250. The gene frequency for B(q), in most of our groups, ranges around 0.250.

Tajiks and Pathans have high p and q, low r.

Hazaras have high q and r.

Uzbegs have high p, low r.

The Tajik and Pathan groups were broken down geographically. The geographical distributions show indications of:

An increase of p Northward
An increase of q Eastward
An increase of r Northward and Westward.

Announcement of the Twenty-second Annual Meeting of The American Association of Physical Anthropologists

At the invitation of the American Anthropological Association and of the University Museum of the University of Pennsylvania the twenty-second annual meeting of the Association will be held in Philadelphia jointly with the Christmas meetings of the American Anthropological Association. The dates proposed are December 27 and 28, 1952, with the latter given over to papers of vital interest both to physical anthropologists and to those whose primary field is social anthropology whether of past peoples (archaeology) or the present (social anthropology proper and ethnology) with linguists included in both subdivisions. The feature of this session will be the Symposium organized by Dr. W. S. Laughlin dealing with methods of determining significant degrees of relationship and evaluating difference between the major disciplines in anthropology as a whole. The chairman of the local committee is Dr. J. L. Angel, secretary of the Association.

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Farris, Dr. Edmond J., The Wistar Institute of Anatomy and Biology, 36th and Woodland Avenue, Philadelphia 4, Pa.

Fejos, Dr. Paul, Wenner-Gren Foundation for Anthropological Research, 14 East 71st Street, New York 21, N. Y.

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HEIZER, DR. ROBERT F., Department of Anthropology, University of California, Berkeley 4, Calif.

HENDERSON, MR. GERALD, Department of Sociology and Anthropology, Brooklyn College, Brooklyn 10, N. Y.

HERSKOVITS, DR. MELVILLE J., Northwestern University, Evanston, Ill.

HERTZBERG, H. E., 418 N. Park Place, Yellow Springs, Ohio.

HESS, DR. LEO, 15 Kenwood Street, Brookline 46, Mass.

HILL, DR. W. C. OSMAN, Zoological Society of London, Regents Park, London, N.W. 8, England.

HJELMAN, DR. GÖRAN, Institute of Dentistry, University of Helsinki, Fabianinkatu 24, Helsinki, Finland.

HOEBEL, DR. E. A., University of Utah, Salt Lake City 1, Utah.

HOOIJER, DIRK ALBERT, Rijksmuseum van Natuurlijke Historie, Leiden, Netherlands.

HOOTON, PROF. E. A., Peabody Museum, Cambridge 38, Mass.

HOWE, DR. HUBERT S., 115 E. 61st Street, New York 21, N. Y.

HOWELL, MR. FRANCIS CLARK, Department of Anthropology, University of Chicago, Chicago 37, Ill.

HOWELLS, DR. W. W., University of Wisconsin, Madison 6, Wis.

HOWORTH, DR. BECKETT, 33 E. 61st Street, New York 21, N. Y.

HULSE, DR. FREDERICK S., Department of Anthropology, University of Washington, Seattle, Wash.

HUNT, DR. EDWARD EYRE, JR., Forsyth Dental Infirmary, 140 The Fenway, Boston 15, Mass.

HURME, DR. V. O., Forsyth Dental Infirmary, 140 The Fenway, Boston 15, Mass.

JAMES, DR. ALICE, Anthropology Division, Hunter College, New York 21, N. Y.

JENSEN, DR. DOROTHY CROSS, Department of Anthropology, Hunter College, 695 Park Avenue, New York City 21, N. Y.

KANSU, PROF. SEVKET AZIZ, Anthropoloji Enstitüsü, Dil ve Tarih-Cografya Fakültesi, Ankara Üniversitesi, Ankara, Turkey.

KAPLAN, MISS BERNICE, 800 Prentis, Detroit 1, Mich.

KAPLAN, DR. E. B., 1001 Grand Concourse, New York 52, N. Y.

KERN, DR. HOWARD M., JR., Department of Anatomy, University of Pennsylvania School of Medicine, Philadelphia 4, Pa.

KEUR, DR. DOROTHY L., 2475 Palisade Avenue, New York, N. Y.

KING, DR. ARDEN R., Middle American Research Institute, Tulane University, New Orleans 15, La.

KLUCKHOHN, DR. CLYDE, Peabody Museum, Cambridge 38, Mass.

KOSKI, DR. KALEVI P., Institute of Dentistry, Fabianinkatu 24, Helsinki, Finland.

KRAHL, DR. V. E., Anatomy Department, University of Maryland, 29 S. Greene Street, Baltimore 1, Md.

KRAUS, MR. BERTRAM S., Department of Anthropology, University of Arizona, Tucson, Ariz.

KROGMAN, PROF. WILTON M., Philadelphia Center for Research in Child Growth, 1701½ Fitzwater Street, Philadelphia 46, Pa.

KROPP, DR. BENJAMIN N., Department of Anatomy, Queen's University, Kingston, Ontario, Canada.

LACHMAN, DR. ERNEST, College of Medicine, University of Oklahoma, 801 E. 13th Street, Oklahoma City 5, Okla.

Lanier, Dr. Raymond R., Department of Radiology, University of Colorado Medical Center, 4200 East 9th Avenue, Denver 7, Colorado.

Lapidus, Dr. Paul W., 1133 Park Avenue, New York 28, N. Y.

Lasker, Dr. Gabriel W., Wayne University College of Medicine, 1512 St. Antoine Street, Detroit 26, Mich.

Laughlin, Dr. William A., University of Oregon, Eugene, Oregon.

Lay, Mr. Tracy, Department of Anthropology, University of California at L. A., Los Angeles 24, California.

Leser, Dr. Paul W., Department of Anthropology, Hartford Seminary Foundation, 55 Elizabeth Street, Hartford 5, Conn.

Lessa, Dr. Wm. A., Anthropology Department, University of California, Los Angeles 24, Calif.

Lesser, Dr. Ethel Boissevain, Castle Point, Hoboken, N. J.

Levine, Dr. Philip, Ortho Research Foundation, Route 29, Raritan, N. J.

Levine, Dr. Victor E., School of Medicine, Creighton University, Omaha, Neb.

Linton, Prof. Ralph, Anthropology Department, Yale University, New Haven, Conn.

Lloyd, Dr. Ruth Smith, Anatomy Department, Howard University, Washington 1, D. C.

Lusterman, Dr. E. A., 165 North Village Avenue, Rockville Centre, Long Island, N. Y.

Mack, Dr. Pauline B., Pennsylvania State College, State College, Pa.

Mainland, Prof. Donald, Department of Preventive Medicine, New York University-Bellevue Medical Center, 477 First Avenue, New York 16, N. Y.

Manter, Dr. John, Anatomy Department, University of Georgia, School of Medicine, Augusta, Ga.

Maranjian, Mr. George, Peabody Museum, Cambridge 38, Mass.

Marzano, Miss Ruth A., Department of Anatomy, University of Chicago Medical School, Chicago 37, Illinois.

Massler, Dr. Maury, University of Illinois College of Dentistry, 808 South Wood Street, Chicago 12, Illinois.

Matson, Dr. G. A., Minneapolis War Memorial Blood Bank, 1914 La Salle Street, Minneapolis 4, Minn.

McCloy, Dr. C. H., Division of Physical Education, State University of Iowa, Iowa City, Iowa.

McCown, Mrs. Elizabeth Richards, 1114 Oxford Street, Berkeley 7, Calif.

McCown, Dr. Theodore D., Anthropology Department, University of California. Berkeley, Calif.

McGregor, Prof. J. H., Columbia University, New York 27, N. Y.

McKern, Mr. Thomas W., Department of Anthropology, University of California, Berkeley 4, Calif.

Meredith, Dr. H. V., School of Health and Physical Education, University of Oregon, Eugene, Oregon.

Michelson, Dr. Nicholas, Veterans Administration, Castle Point, N. Y.

Midlo, Dr. Charles, 1404 Louisiana Avenue, New Orleans 15, La.

Miles, Miss Meryl, Anatomy Department, University of Wisconsin, Madison 6, Wis.

Miles, Miss Suzanna, Peabody Museum, Cambridge 38, Mass.

Misura, Mr. Emil M., Marine Corps Depot, Philadelphia, Pa.

Moloy, Dr. H. C., 40 E. 72nd Street, New York 21, N. Y.

Montagu, Dr. M. F. Ashley, Cherry Hill Road, Princeton, N. J.

Moorrees, Dr. Coenraad F. A., Forsyth Dental Infirmary, 140 The Fenway, Boston 15, Mass.

Moss, Dr. Melvin L., Anatomy Department, Columbia University College of Physicians and Surgeons, 630 West 168th Street, New York City 32, N. Y.

Muller, Dr. Léon, 7 Fresez Street, Luxembourg.

Murphy, Mrs. Yolanda, 572 Amsterdam Ave., New York 24, N. Y.

Mustakallio, Dr. Martti J., Institute of Dentistry, University of Helsinki, Fabianinkatu 24, Helsinki, Finland.

Näätänen, Dr. Esko Kalervo, Department of Anatomy, University of Helsinki, Siltavuorenpenger 20, Helsinki, Finland.

Neumann, Dr. Georg K., Department of Anthropology, Indiana University, Bloomington, Ind.

Newman, Dr. Marshall T., U. S National Museum, Washington 25, D. C.

Newman, Dr. Russell W., Climatic Research Laboratory, Lawrence, Mass.

Noback, Dr. Charles R., Anatomy Department, College of Physicians and Surgeons, 630 W. 168th Street, New York 32, N. Y.

Nourse, Miss Dolores Van C., c/o Dr. Ralph Solecki, Smithsonian Institution, Washington 25, D. C.

Opdyke, Dr. Donald, 5232 Ralph Avenue, Cincinnati 38, Ohio.

Orttung, Mrs. Jane Raven, Department of Anthropology, American Museum of Natural History, New York 24, N. Y.

Osborne, Mr. Richard H., 29 Oaklynn Place, Glen Rock, N. J.

Papez, Prof. James W., Laboratory for Biological Research, 1960 West Broad Street, Columbus 15, Ohio.

Pauls, Mr. Frank, Division of Public Health Laboratories, Box 960, Anchorage, Alaska.

Pedersen, Dr. P. O., National Dental College of Denmark, 4 Universitetsparken, Copenhagen, Denmark.

Pesonen, Dr. Niilo N., Institute of Anatomy, University of Helsinki, Fabianinkatu 24, Helsinki, Finland.

Philippas, Dr. George G., 8 Hippokratous Street, Athens, Greece.

Planansky, Dr. Karel, Psychiatric Institute, 722 W. 168th Street, New York 32, N. Y.

Plough, Prof. H. H., Amherst College, Amherst, Mass.

Pollitzer, Mr. William, International House, 500 Riverside Drive, New York City.

Rabkin, Dr. Samuel, Interlochen, Mich.

Racher, Mrs. Alice Bro, 711 North 41st Avenue, Omaha, Neb.

Rainey, Dr. Froelich, University Museum of the University of Pennsylvania, 33rd and Spruce Streets, Philadelphia 4, Pa.

Reed, Dr. Charles A., University of Illinois College of Pharmacy, 808 S. Wood Street, Chicago 12, Ill.

Reed, Dr. Erik K., 238 Griffin Street, Santa Fe, N. M.

Reed, Prof. Lowell J., School of Hygiene and Public Health, Johns Hopkins University, Baltimore 5, Md.

REITER, DR. PAUL, Department of Anthropology, University of New Mexico, Albuquerque, N. M.

REYBURN, MRS. W. D., Box 776, Reidsville, North Carolina (Marie Fetze).

REYNOLDS, DR. EARLE L., Atomic Bomb Casualty Commission, APO no. 182 c/o Postmaster, San Francisco, California.

RIFE, DR. DAVID C., Department of Zoology, Ohio State University, Columbus 10, Ohio.

RILEY, MR. CARROLL L., Department of Anthropology, University of New Mexico, Albuquerque, New Mexico.

ROBINSON, DR. J. T., Transvaal Museum, P.O. Box 413, Pretoria, South Africa.

ROBINSON, MR. ROBERT G. H., Department of Anthropology, University of Oregon, Eugene, Oregon.

ROCHE, DR. MAURICE B., 3720 Washington Blvd., St. Louis, Mo.

ROGERS, PROF. S. L., 920 Martinez Street, San Diego 6. Calif.

RONEY, DR. JAMES G., 1090 Grizzly Peak Boulevard, Berkeley 8, California (now removed to Iran).

ROWE, DR. CHANDLER W., Department of Anthropology, Lawrence College, Appleton, Wisconsin.

SALZMANN, DR. J. A., 654 Madison Avenue, New York, N. Y.

SANGHVI, DR. L. D., Indian Cancer Research Centre, Bombay 12, India.

SANTIANA, DR. ANTONIO, Museo Etnografico, Universidad Central, Quito, Ecuador.

SAUNDERS, DR. J. B. DEC. M., Anatomy Department, Medical School, University of California, Parnassus and 2nd Avenues, San Francisco 22, Calif.

SAVARA, DR. BHIM, University of Oregon Dental School, Eugene, Oregon.

SAWIN, DR. PAUL B., Roscoe B. Jackson Memorial Laboratory, P.O. Box 847, Bar Harbor, Maine.

SCHAEFFER, DR. BOBB, Department of Palaeontology, American Museum of Natural History, Central Park West at 79th Street, New York City 24, N. Y.

SCHULTZ, PROF. ADOLPH H., Anthropologisches Institut der Universität, Sempersteig 3, Zürich, Switzerland.

SCHUMAN, DR. ELIHU LEON, 51 Baker Avenue, Lexington, Mass.

SCOTT, PROF. DONALD, 21 Kirkland Street, Cambridge 38, Mass.

SCOTT, DR. J. P., Roscoe B. Jackson Memorial Laboratory, P. O. Box 78, Bar Harbor, Maine.

SEIB, DR. GEORGE, 2323 Lafayette Avenue, St. Louis, Mo.

SELTZER, DR. CARL C., 13 Holyoke Street, Cambridge 38, Mass.

SENSENIG, DR. E. CARL, Department of Anatomy, Medical College of Alabama, Birmingham 5, Ala.

SENYÜREK, PROF. MUZAFFER SÜLEYMAN, Antropoloji Enstitüsü, Dil ve Tarih-Coğrafya Fakültesi, Ankara Üniversitesi, Ankara, Turkey.

SHADE, DR. CHARLES I., Anthropologist, C.I.U., AGRS, APO no. 3, c/o Postmaster, San Francisco, California.

SHANKLIN, DR. WM. M., American University, Beirut, Lebanese Republic.

SHAPIRO, DR. H. H., Anatomy Department, Columbia College of Physicians and Surgeons, 630 W. 168th Street, New York 32, N. Y.

SHAPIRO, DR. H. L., American Museum of Natural History, New York 24, N. Y.

SILLMAN, DR. J. H., 667 Madison Ave., New York, N. Y.

Singer, Dr. Ronald, Department of Anatomy, University of Cape Town, Cape Town, South Africa.

Škerlj, Dr. Božo, Resljeva 13, Ljubljana, Yugoslavia.

Smith, Dr. R. Dale, Department of Anatomy, Creighton University School of Medicine, Omaha 2, Nebraska.

Snodgrasse, Dr. R. M., Graduate School of Medicine, University of Pennsylvania, Philadelphia 4, Pa.

Snow, Dr. Charles E., Department of Anthropology, University of Kentucky, Lexington 29, Ky.

Sontag, Dr. L. W., Fels Research Institute for the Study of Human Development, Antioch College, Yellow Springs, Ohio.

Spuhler, Dr. James N., Institute of Human Biology, University of Michigan, Ann Arbor, Mich.

Stagg, Mr. Frederick L., Peabody Museum, Cambridge 38, Mass.

Steedman, Prof. Elsie V., Anthropology Division, Hunter College, New York 21, N. Y.

Stein, Dr. M. Russell, 157 W. 57th Street, New York, N. Y.

Stekelis, Prof. M., Department of Prehistoric Archaeology, Hebrew University, Jerusalem, Israel.

Stewart, Dr. T. D., U. S. National Museum, Washington 25, D. C.

Strandskov, Dr. Herluf H., Zoology Department, University of Chicago, Chicago 37, Ill.

Strange, Dr. Howard, 2376 E. 71st Street, Chicago 49, Ill.

Straus, Dr. William L., Jr., Professor of Physical Anthropology, Biology Building, Johns Hopkins University, Baltimore 18, Md.

Sullivan, Dr. Walter E., Department of Anatomy, University of Wisconsin, Madison, Wis. Philadelphia Center for Research in Child Growth, 1701½ Fitzwater Street, Philadelphia 46, Pa.

Tappen, Dr. Neil C., Institute of Human Biology, University of Michigan, Ann Arbor, Michigan.

Telkkä, Dr. Antti J., Department of Anatomy, University of Helsinki, Siltavuorenpenger 20, Helsinki, Finland.

Terry, Prof. Robert J., School of Medicine, Washington University, St. Louis 10, Mo.

Thieme, Dr. Frederick, Department of Anthropology, University of Michigan, Ann Arbor, Mich.

Thomsen, Dr. Selma Ø., Forsyth Dental Infirmary, 140 The Fenway, Boston 15, Mass.

Titiev, Dr. Mischa, 2604 Brockman Blvd., Ann Arbor, Mich.

Townsley, Dr. William, 306 Shankill Road, Belfast, Ireland.

Trotter, Dr. Mildred, Anatomy Department, Washington University, 4580 Scott Avenue, St. Louis 10, Mo.

Truex, Dr. R. C., Anatomy Department, Hahnemann Medical College, 235 N. 15th Street, Philadelphia 2, Pa.

Van Stone, Mr. James, Department of Anthropology, Box 5, University of Alaska, College, Alaska.

Van Wart, Lt. Franklin D., Aero-Medical Laboratory, Wright Air Development Center, Dayton, Ohio.

von Bonin, Dr. Gerhardt, Anatomy Department, University of Illinois, 1853 W. Polk Street, Chicago 12, Ill.

Washburn, Dr. S. L., Department of Anthropology, University of Chicago, Chicago 37, Ill.

Wasser, Mr. Roger G., Department of Anthropology, University of Arizona, Tucson, Arizona.

Watkins, Dr. Mark Hanna, Howard University, Washington 1, D. C.

White, Dr. Robert, Climatic Research Laboratory, Lawrence, Mass.

Wiener, Dr. Alexander S., 64 Rutland Road, Brooklyn 25, N. Y.

Wilber, Dr. C. G., Biological Laboratories, St. Louis University, 1402 S. Grand Blvd., St. Louis 4, Mo.

Wolffson, Davida M., Department of Anthropology, University of Chicago, Chicago 37, Ill.

Womble, Mr. William H., Department of Anthropology, University of California at L. A., Los Angeles 24, Calif.

Woo, Dr. Ju-kang, Department of Anatomy, Dairen University School of Medicine, Dairen, China.

Wormington, Miss H. M., Denver Museum of Natural History, City Park, Denver 6, Colorado.

Wright, Dr. Harry B., 616 Medical Arts Building, Philadelphia, Pa.

Wulsin, Dr. F. R., Sociology Department, Tufts College, Medford 55, Mass.

Life Members

Andrews, Dr. James M., 4860 Linnean Avenue, N. W., Washington 8, D. C.

Ball, Dr. Robert P., 410 Convention Street, Baton Rouge, Louisiana.

Bodel, Mr. John K., Jr., Hotchkiss School, Lakeville, Conn.

Briggs, Mr. L. Cabot, Hancock, N. H.

Bronner, Prof. Finn J., New York University, 209 E. 23rd Street, New York, N. Y.

Cobb, Dr. W. Montague, Anatomy Laboratory, Howard University, Washington 1, D. C.

Danforth, Prof. Charles H., Anatomy Department, Stanford University, Calif.

Deignan, Dr. Stella Leche, 2236 Decatur Place, N. W., Washington 8, D. C.

Fortuyn, Dr. A. B. D., Beeklaan 454 E, den Haag, Holland.

Gower, Prof. Charlotte D., (address not known).

Hughes, Dr. Byron O., University of Michigan, Ann Arbor, Mich.

de Jonge Cohen, Dr. Th. E., University of Amsterdam, Jan Luykenstraat 43, Amsterdam, Zuid, Holland.

Loo, Dr. Yu Tao, Chinese Association for the Advancement of Science, 235 Shensi Road (Southern), Shanghai, China.

Mills, Dr. C. A., Cincinnati General Hospital, Cincinnati 29, Ohio.

Morton, Dr. Dudley J., Anatomy Department, Columbia University, 630 W. 168th Street, New York 32, N. Y.

Oetteking, Dr. Bruno, Museum of American Indian, Broadway at 155th Street, New York 32, N. Y.

Phillips, Dr. T. W., Hahnemann Medical College, Philadelphia, Pa.

Pinkely, Dr. George, (address not known).
Post, Dr. Richard H., Department of State, Washington, D. C.
Redway, Dr. Laurance D., 82 S. Highland Avenue, Ossining, N. Y.
Stevenson, Dr. Paul H., Mental Hygiene Division, U. S. Public Health Service, Washington 25, D. C.
Tucker, Dr. Wm. B., Veterans Administration Hospital, 54th Street and 48th Avenue, Minneapolis 6, Minn.
Williams, Dr. George Dee, Lawson V. A. Hospital, Chamblee, Ga.
Woodbury, Dr. Robert M., (address not known).
Zwemer, Dr. R. L., 5003 Battery Lane, Bethesda, Md.

Checklist of Australopithecine Remains.— The following list has been compiled from Table 1, p. 163, of Wilhelm Ehgartner's summary article "Fossil Menschenaffen aus Südafrika. Australopithecinae. Die bisherigan Funde und ihre Bedeutung für die Stammesgeschichte der Menschheit" (Mitt. Anthrop. Ges. Wien, v. 80, no. 3, 1950, pp. 157–212).

Australopithecus africanus Dart. Type locality: Taungs, Bechuanaland.

1. Part of brain case with base, face with mandible, skull impression; child; found late 1924.

Plesianthropus transvaalensis Broom. Type locality: Sterkfontein, Transvaal.

1. Skull impression, parts of base, parietal, occiput, and right and left maxillae with teeth (S 1); adult female; found August, 1936.
2. Parts of 3 skulls: Maxilla (S 2), fragmentary skull (S 3), left maxilla (S 4), mandibular symphysis; adults, 2 males, 1 female; found between 1936 and 1939.
3. Left femur (distal end), os capitatus of right hand, proximal part of phalanx of finger; adult; found 1938.
4. Part of maxilla with teeth; subadult; part of maxilla with milk dentition; child; found April 8, 1947.
5. Well preserved, nearly complete skull, without mandible (S 5); adult female; found April 18, 1947.
6. Nearly complete mandible; parts of right scapula; proximal end of right humerus, 1 well preserved vertebra; several rib fragments; adult male; found June 24, 1947.

7. Pelvis (without sacrum), left femur (proximal end), parts of tibia, ribs and vertebrae; parts of badly preserved skull; adult female; found August 1, 1947.

8. Nearly complete mandible; adult male; found August 10, 1947

9. Parts of 3 more skulls (S 6–8); found September-December, 1947.

Including the smaller skull fragments not mentioned here there are so far 5 well preserved, and 8 less well preserved Plesianthropus skulls, and around 200 single teeth.

Paranthropus robustus Broom. Type locality: Kromdraai, 2 miles from Sterkfontein, Transvaal.

1. Most of maxilla, left half of face, nearly complete left zygomatic arch, part of left parietal, left side of skull base, most of right mandibular ramus, with teeth, right humerus (distal end), right ulna (distal end), 2 metacarpals of left hand, 2 finger bones, right talus; adult male; found 1938.

2. Part of mandible with milk dentition and first permanent molar; female child; found 1941.

Paranthropus crassidens Broom. Type locality: Swartkrans, 1 mile from Sterkfontein.

1. Two nearly complete skulls, 3 nearly complete mandibles; adult; 1 nearly complete skull, 2 nearly complete mandibles; children; 6 less complete skulls and mandible, and about 300 single teeth; found 1948–1950.

Australopithecus prometheus Dart. Type locality: Makapansgat, Central Transvaal.

1. Most of occiput with adjacent parts of parietal; subadult female; found September 1947.

2. Part of palate; adult female; found September, 1947.

3. Mandible with teeth; subadult male; found July, 1948.

4. Craniofacial fragment (consisting of right maxilla, nasal bones, adjoint parts of frontal, 2 premolars and 2 molars); adult female; found autumn, 1948.

5. Nearly complete left ilium and right ischium; subadult male; found autumn, 1948.

Telanthropus capensis Broom. Type locality: Swartkrans.

1. Nearly complete mandible with teeth; adult; found, 1949.

AGE CHANGES IN THE POSITION OF THE OCCIPITAL CONDYLES IN THE CHIMPANZEE AND GORILLA

E. H. ASHTON AND S. ZUCKERMAN
Department of Anatomy, University of Birmingham, England

TWO FIGURES

INTRODUCTION

The differences that occur in the relative growth of the facial skeleton and cranium after birth have been the subject of numerous inquiries (e.g., Keith, '10; Krogman, '31a, b, c). The difference is most marked during the period of eruption of the permanent teeth, when the face grows very rapidly, and is particularly conspicuous in those primate species, for example baboons and gorillas, that are furnished with a powerful set of permanent teeth (e.g., Zuckerman, '26). It is usually associated with a change in the inclination of the plane of the foramen magnum relative to the basi-cranial axis, and with a so-called ''backward migration'' of the occipital condyles. In so far as the position of the occipital condyles relative to the atlas is fixed throughout life, this so-called ''migration'' should be regarded as an expression of the change in the relative proportions of the post-condylar and pre-condylar parts of the skull.

The ratio of the lengths of these two segments projected along the Frankfort horizontal has been used as a measure of the balance of the whole skull on the vertebral column (Schultz, '42), and has also been related to the poise of the head in relation to the vertebral column (Le Gros Clark, '50). Estimates of the mean and standard deviation of this ''condylar-position index'' in adult specimens of 11 primate species are given by Ashton and Zuckerman ('51), but to the best of our knowledge no details are available to show how the index varies with age. The present note provides information on this subject for the chimpanzee and gorilla.

MATERIAL AND METHODS

Skulls

One hundred and eighty-two gorilla and 163 chimpanzee skulls belonging to the Powell-Cotton Museum, Birchington, were studied. Their sexes were known from field records.

Age groups

The skulls were divided into the following age groups:

1. All skulls up to, and including the full milk dentition.
2. Skulls with the complete milk dentition, and only the first molars of the permanent dentition.
3. Skulls containing only the first molars and incisors of the permanent dentition, the other teeth being deciduous.
4. All skulls intermediate in age between those defined in groups 3 and 5.
5. Skulls possessing a full complement of permanent teeth, and in which attrition was apparent on not more than the two lingual cusps of the upper second molar.
6. Skulls possessing a full permanent dentition, but showing wear on more than the two lingual cusps of the upper second molar.

Biometric technique

Measurement. The dimensions and methods of measurement employed in this study were identical with those described in an earlier paper (Ashton and Zuckerman, '51).

Statistical analysis. The data were first analyzed separately for the different age and sex subgroups of both species of ape. Sex differences within each age group of chimpanzee proved to be slight (no sex difference within any group reached even the 10% level of significance). Male and female skulls of corresponding age groups were therefore pooled. For the same reason all data relating to chimpanzees of age groups 5 and 6 were pooled.

In the gorilla skulls there were no significant sex differences in the index in age groups 1 to 4 ($P > 0.05$), and the data for male and female skulls in each of these groups were therefore also pooled. Significant sex differences ($P < 0.02$) did, however, appear in group 5. The final grouping of the data was thus: Both sexes of chimpanzee for age groups 1, 2, 3 and 4, and a 5th group comprising both sexes of age groups 5 and 6. In the gorilla the grouping was: both sexes for age group 1, 2, 3 and 4, and the males and females separately for age groups 5 and 6 respectively.

TABLE 1

Age changes in the condylar-position index of the chimpanzee and gorilla. For explanation of age and sex subgroups see text

AGE SUBGROUP	CHIMPANZEE				
	1	2	3	4	5–6
Mean	50.3	41.1	35.5	30.6	24.9
No. of skulls	14	25	17	11	96
S.D.	5.4	6.2	3.1	3.4	3.4
S.E. Mean	1.44	1.25	0.74	1.02	0.35

SUBGROUP AGE AND SEX	GORILLA							
	1	2	3	4	5 M	5 F	6 M	6 F
Mean	51.7	43.0	31.3	32.1	24.9	27.4	29.8	26.4
No. of skulls	18	21	15	15	32	36	28	17
S.D.	6.8	5.6	5.2	4.6	4.2	3.9	7.2	4.4
S.E. Mean	1.61	1.23	1.35	1.18	0.74	0.64	1.35	1.06

RESULTS

The results are set out in table 1 and are shown in graphical form in figure 1. In no instance does the standard error of the mean value of the index exceed 5% of the mean.

In the chimpanzee, the value of the index falls steadily from stage 1 to stage 5–6, the differences between successive age groups averaging 6.4 units, and all differences between successive age groups being statistically significant at a level of $P < 0.01$.

In the gorilla, the value of the index falls from 52 in group 1 to 31 in group 3, the differences between groups 1 and 2 and between groups 2 and 3 being significant at a level of $P < 0.001$. The index is practically the same in groups 3 and 4, but is much smaller in group 5, the difference of 7.2 units in the case of the male, and 4.7 units in the case of the female being significant at the 0.1% level.

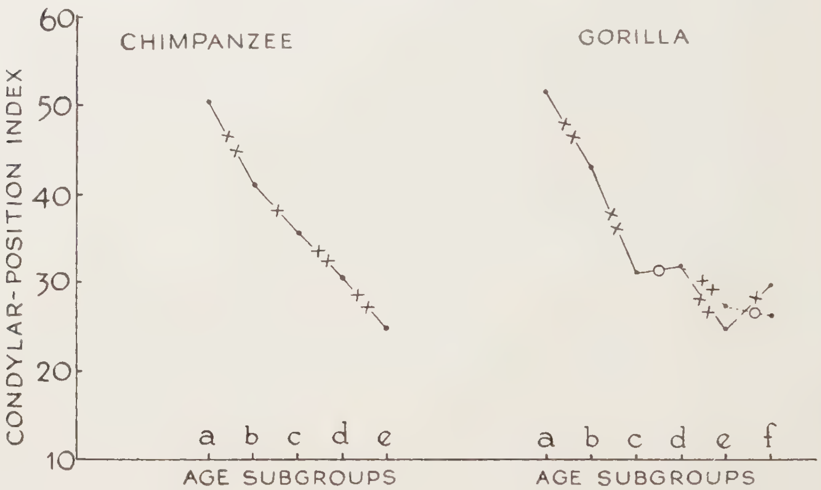

Fig. 1 Age-changes in the condylar-position index of the chimpanzee and gorilla.

The difference between successive age groups is significant at a level of

x $P \leq 0.01$ xx $P \leq 0.001$

o indicates a negligible difference.

For explanation of age subgroups see text.

As already observed, there are no age or sex differences in the index of the chimpanzee after the eruption of the third permanent molars. In the female gorilla, the change between groups 5 and 6 is approximately one unit only, and is not significant at the 2% level. The index in the adult male gorillas of group 6 is, on the other hand, 4.9 units greater than in the adult male gorillas of group 5, the change being significant at the 1% level.

DISCUSSION

These findings reflect the fact that from birth until all the permanent teeth have erupted, the face of apes grows relatively more than the cranium. The increase in the value of the condylar-position index in the old adult male gorilla reflects the development of a keel at the confluence of the nuchal and sagittal crests, an osseous change which is associated with an increase in the area of origin of the temporalis muscle and of the nuchal musculature. Krogman ('31a) has illustrated the same change in his study of age changes in the pre- and post-porionic dimensions of the gorilla skull.

In a study of specimens of monkeys, apes and men, Schultz ('42), measuring from the center point of the occipital condyles, found that the "lengths of the leverarms to prosthion and to inion are in inverse relation to the weights used in balancing the head from the same points with only a small discrepancy between the empirically found and theoretically expected results." The condylar-position or "head-balancing index," derived from measurements of the skull, thus furnishes a good indication of the balance (or imbalance) of the head, when regarded as dead-weight, and when supported on the occipital condyles, and orientated in the Frankfort plane. It also provides a relative measure of the force which must be supplied by the nuchal musculature in order to maintain the poise of the head in the ear-eye horizontal — the heads of presumably all primates, as Schultz has shown, being heavier in front of the occipital condyles than behind, when oriented in this plane.

It is not clear, however, that this index, derived as it is from studies of isolated skulls (or heads), provides an exact measure of the way the head is carried in the living animal. We say this because in spite of age differences in the value of the index, it is our impression that there are no striking, or indeed, obvious differences in the gait of young and adult great apes, nor in the way they carry their heads. The precise numerical relation of the occipital condyle-prosthion and

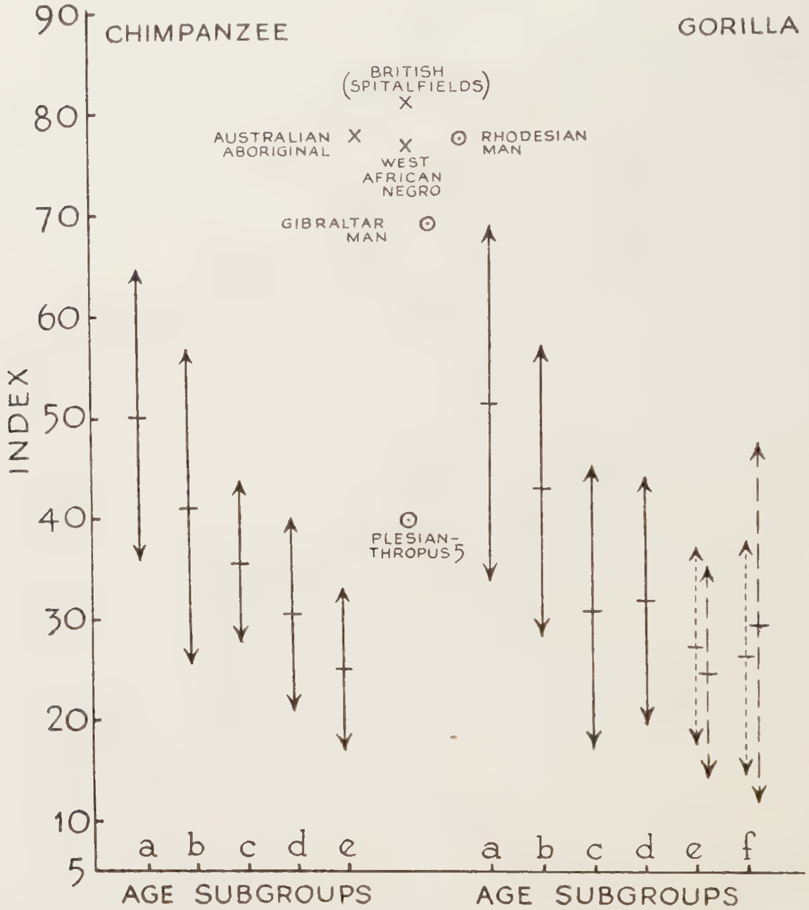

Fig. 2 The value of the condylar-position index in various hominoids.

Mean values for the chimpanzee and gorilla are represented by transverse bars, the 98% fiducial limits being indicated by arrows.

x indicates average values for three types of modern man.

⊙ indicates values for single specimens of fossil man and *Plesianthropus.*

For explanation of age subgroups see text.

——— Both sexes ..... Female – – – Male

occipital condyle-inion dimensions would hardly seem, therefore, to be of much value in showing how the head is poised on the vertebral column, or whether an animal is quadrupedal or walks upright. These matters depend far more on the nature of the nuchal musculature, on the curvature of the spinal column, and on the carriage of the trunk on the hind limbs.

Our previous study (Ashton and Zuckerman, '51) showed that the value of the condylar-position index in the australopithecine fossil *Plesianthropus* 5 is significantly greater than in the adult chimpanzee (at the 2% level of significance), whereas it easily falls into the 98% range for the adult gorilla (males and females pooled). Comparisons have now been made with each age group of the two apes (fig. 2). The value of the index in *Plesianthropus* does not differ from that in chimpanzees of age groups 1, 2, 3, but is significantly greater ($P < 0.02$) than in chimpanzees of groups 4 and 5–6. It agrees (that is to say, the maximum difference does not exceed the 10% level of significance) with the value of the index in gorillas up to age group 4. It is significantly greater ($P < 0.02$) than in male and female gorillas of group 5 and female gorillas of group 6. The mean value of the index in male gorillas of group 6 is less than it is in *Plesianthropus* 5, but the difference does not exceed the 10% level of significance.

Since it is to be expected that the values for adult hominid skulls would be closer to the range for apes, of any age, than would those of infantile human skulls, we have re-examined, for purposes of comparison, the values we obtained before (Ashton and Zuckerman, '51), for the following three series of adult human skulls; as well as for the two fossil hominids Rhodesian Man and the Gibraltar Neanderthal type.

	VALUE OF CONDYLAR-POSITION INDEX
30 Englishmen (Spitalfields)	81.3 ± 1.70
28 Australian aboriginals	78.3 ± 1.24
29 West African Negroes	77.0 ± 1.30
Rhodesian Man (cast)	78
Gibraltar Man (cast)	66

It may be noted that Schultz ('42) gives the values of 76 and 63 for the Rhodesian and Gibraltar skulls when the posterior limb of the index is taken to the inion and not, as we have done, to the opisthocranium.

In all cases except the Gibraltar fossil, the value of the index in the hominid skulls is significantly greater ($P < 0.001$) than that characterizing every age of chimpanzee or gorilla, and including even those possessing only milk teeth. The value for the Gibraltar skull was significantly greater than the mean index for all gorillas and chimpanzees in which the permanent teeth had started erupting, but while much bigger than the mean value for gorillas possessing only milk teeth (66 as compared with 52), the difference was not significant at the 5% level.

We have not been able to determine values for the index in skulls assigned to the genus *Pithecanthropus,* but it is worth noting that, according to Weidenreich ('43), this group of fossil hominids does not differ essentially from modern man in the relative position of the occipital foramen. Weidenreich regarded "the more central position of the occipital foramen, in obvious contrast to the conditions in anthropoids, as a specific hominid character."

It thus follows that whereas the values of the condylar-position index in hominid skulls differ greatly from those of apes of all ages, that for *Plesianthropus* 5 falls well within the range of all gorillas not possessing a full complement of permanent teeth, and also within the range for adult male gorillas. The value of the condylar-position index in this fossil also corresponds far more closely with the values for the chimpanzee than it does with those for human skulls.

We are again indebted to Mrs. H. B. Powell-Cotton for permission to study skulls in her collection. Expenses incurred in the "field-work" were met by a grant from the Wenner-Gren Foundation, U.S.A., and in computing by a grant from the Royal Society of London.

SUMMARY

1. The condylar-position index has been determined for 182 gorilla and 163 chimpanzee skulls. The skulls were assigned to 6 age groups according to the stage of dentition.
2. The index in the chimpanzee falls steadily from stage 1, the period of the deciduous dentition, to stage 6, when all the permanent teeth are in place and some are greatly worn. All differences between successive age groups are statistically significant at a level of $P < 0.01$.
3. Age changes in the gorilla are similar, except that the index in old adult males is significantly greater than in young adult males, owing to the development of a keel at the confluence of the nuchal and sagittal crests.
4. The precise numerical value of the index appears to give little indication of the poise of the head on the vertebral column, or of an animal's gait.
5. The value of the index in various hominid skulls differs greatly from that of apes of all ages, whereas that for *Plesianthropus* 5, an australopithecine skull, falls well within the range of young and even old anthropoid apes.

ADDENDUM

In a note published in the March, 1952, issue of this Journal, Le Gros Clark makes certain remarks about our previous observations on the position of the occipital condyles (Ashton and Zuckerman, '51), which also bear on our present findings. He refers to the need, which we fully endorse, to make sure that quantitative comparisons are based on measurements which are morphologically comparable. This we appear to have done in the comparisons relating to two of the three indices where our findings bear out conclusions that he himself had reached, but he implies that we have not done so, and questions our methods and reasoning with respect to the third comparison, that relating to the position of the occipital condyles, where we disagree with his interpretations.

The essential point in which our previous findings disagreed with Le Gros Clark's ('50) generalizations was in their failure to confirm the impression he clearly gives (pp. 244–246) that the value of the condylar-position index in *Plesianthropus* 5 is close to the human range and outside that encountered in the great apes. It is true that on page 256 he observes that the occipital condyles "though occupying a more forward position on the base of the skull than in the large apes, are by no means so far forward relative to the total skull-length as they normally are in a modern human skull." He does not, however, provide any quantitative estimates for the value of the index in man, nor does he point out that the value of the condylar-position index is actually very much closer to that of the sub-human primates than to that of the Hominidae. This is the central fact which emerged from our previous study.

Le Gros Clark writes that we are mistaken in our views about the significance of the condylar-position index as a measure of the balance of the skull. He states, apparently *ex cathedra,* that the center of gravity of the skull, relative to the occipital condyles, will not be altered "to any appreciable extent" by the backward prolongation of the sagittal and occipital crests, and he criticizes us, therefore, for including this posterior extension in our estimates of the length of the post-condylar segment. In point of fact, as Schultz ('42) has shown by direct measurements of macaques, chimpanzees and men, the ratio we have used makes it possible to predict "with very considerable accuracy the ratio between the weights needed to balance the head at the corresponding landmarks." In any event, it is difficult to understand how the considerable posterior extension which may develop in the skull of the adult male gorilla, and the masses of muscle attached to it, would fail to influence the balance of the skull on the occipital condyles when, as in the measurements Schultz, Le Gros Clark, and ourselves have taken, the skull is considered in isolation. As we have, however, pointed out on page 283, the fact that conspicuous age differences occur in the values of the condylar-position index in the great apes, in spite of an apparent absence of any striking differences in the gait of young or adult great apes, or in the way they carry their heads, makes it possible that inferences about the carriage of the head derived from a study of the precise values of the condylar-position index in isolated skulls may be misleading.

A further criticism made by Le Gros Clark relates to the fact that in his estimates of the condylar-position index he measured from the point of maximum convexity of the condyle, whereas our reference point was the lowermost point of the condyle with the skull orientated in the Frankfort horizontal. In comparison with his own figures this may occasionally lead us to an exaggerated estimate of the relative length of the post-condylar segment, particularly in adult male gorillas. But a test that we have carried out does not suggest that any differences for which this could account makes it necessary for us to qualify our general conclusions about the position of the occipital condyles. As it is, we based our estimate of the condylar-position index in the Sterkfontein skull on the same orientation of the skull that we used for the great apes. If the Frankfort horizontal is used to indicate the plane in which the head is carried (as in Le Gros Clark's, Schultz's and our own studies), it would surely be more correct to treat the lowermost point, rather than the point of main convexity, of the condyles as the fulcrum of the skull, *when studied in isolation.*

Regardless of these considerations, or of statistical niceties, it is perfectly obvious from figure 2 of our present paper that the value of the index in *Plesianthropus* 5 falls far more closely into the population of points for the great apes than for the Hominidae. By emphasizing the issues he does in his short note, Le Gros Clark diverts the discussion from the main point of our previous paper, and one which is corroborated in the present one, that the value of the condylar-position index in *Plesianthropus* 5, whether or not it is taken in combination with other indices, resembles that in apes more than in man, and not the other way round.

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Antiquity of Homo Sapiens.— That *Homo sapiens* dates back at least to the Mindel-Riss interglacial period, of Middle Pleistocene Age, would be no surprise to vertebrate paleontologists, who are familiar with the evidence that the latest, or almost latest, stages of evolution of many mammals had been reached in the late Pliocene or Pleistocene epochs. But, as Weidenreich (1937, 1946) has so fully shown, the fact that the earlier representatives of *Homo sapiens* were contemporaneous with the Neanderthal race does not neutralize the evidence of comparative osteology that the Neanderthal stage retained many relatively primitive features which were gradually lost in the more advanced members of *Homo sapiens.*— W. K. Gregory. Evolution Emerging. Macmillan, 1951, 2 vol.

FLUORINE AND OTHER CHEMICAL TESTS OF SOME NORTH AMERICAN HUMAN AND FOSSIL BONES[1]

ROBERT F. HEIZER AND SHERBURNE F. COOK
Departments of Anthropology and Physiology, University of California

The authors have been interested for several years in the broad topic of the fossilization of human bone, and have published several papers (Cook and Heizer, '47; Heizer and Cook, '49; Cook, '51a, '51b; Cook and Heizer, *n.d.)* detailing results of chemical analyses of bones primarily from California with discussions of the possibility and in the hope that these data might prove to be of use in establishing relative or absolute archaeological chronologies. The potentiality of investing such chemical determinations with the factor of time has long been recognized, and numerous independent attempts to develop a method have been made in the past. As we have proceeded along the lines of our own investigation, we have from time to time checked other methods such as that of Gangl ('36) who analyzed archaeological bone for fat (cf. Cook and Heizer, '47), and of Thunberg ('47) who determined the citric acid content of medieval and prehistoric bones from Europe.[2]

We report here on our findings primarily with reference to a third special method, that of the fluorine content of archaeological bone. The origin of this method is somewhat obscure, though the first real demonstration was in 1844 by a British chemist, John Middleton (1844a, b, c). Half a century later a French mineralogist, Adolf Carnot, made a large number of chemical determinations of paleontological bones, and published his results in three brief notes and one extended article (Carnot, 1892a, b, c, 1893a). Carnot's analysis of bones from different geological horizons indicated that their average fluorine content increased with their age. Taking the fluorine-apatite ratio as unity or 1.0, Carnot found that the average fluorine content of Recent or Post-Pleistocene bones was .058; of Pleistocene bones, .360; of Tertiary bones .620; of Mesozoic bones, .907 and of bones from the Paleozoic, .993 (Carnot, 1893a, table).

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[1] The authors acknowledge, with thanks, for technical aid and assistance in supplying materials for analysis, the following: C. L. Camp, Department of Paleontology, Berkeley; E. W. Gifford, Director of Anthropological Museum, Berkeley; G. W. Hewes, University of Southern California; T. D. McCown, Department of Anthropology, Berkeley; T. D. Stewart and M. T. Newman, U. S. National Museum. This work was carried out with funds supplied by the Wenner-Gren Foundation for Anthropological Research (Grant No. 207), for which we express our appreciation to Dr. Paul Fejos and the Board of Directors.

[2] Our check determinations are not yet completed.

The process by which fluorine ions present in the ground water combine with the hydroxyapatite of buried bones to form the stable compound fluorapatite is described elsewhere (Oakley, '48, '50; Heizer, '50, p. 11), as is the laboratory method for determining the fluorine content of the bone. Carnot's analytical method is described in Carnot (1893b); that used by us and others (e.g., Olsen, '50) is detailed by Willard and Winter ('33). Because the amount of fluorine present in ground waters will vary geographically, the fluorine content of buried bones from different regions or even from different locations in the same region will therefore vary in relation to the two factors of antiquity and fluorine level of ground water. No hope is entertained, therefore, that a general dating curve based on fluorine content of bones can be secured. The method, further, will not yield an absolute time reckoning method. The one outstanding application of the method, seen first and applied by Carnot (1893a, pp. 192–193), and later by Oakley and his colleagues (Oakley, '48, '50a, b; Oakley and Montagu, '49; Montagu and Oakley, '49; Oakley and Hoskins, '50), is in those cases where bone artifacts or human remains are recovered in apparent association with the bones of extinct animals. Often human remains are recovered under conditions where it is impossible to make the required stratigraphic observations with reference to possible intrusion or chance association, so that it cannot be stated that the human bones lay in a deposit whose immediately superincumbent layers were undisturbed by an intrusive pit. This situation of doubt, primarily ascribable to excavation unaccompanied by proper stratigraphic observation, is the chief cause of the controversy over a number of possibly ancient human skeletal finds from North America such as the Minnesota girl, Tepexpan man, Vero and Melbourne skeletons, and others. Since most human skeletal remains found are those of intentionally buried individuals whose bones lie in anatomical articulation, the presumption is, generally speaking, that a grave was dug and that the skeleton is intrusive. Many times a grave pit two or three feet deep will penetrate deposits of Pleistocene age. Under ordinary circumstances we find human burials in obvious association with other cultural remains such as occupation trash deposits, charcoal from hearths, and the like, so that no reasonable doubt is raised as to the time of deposition of the skeleton and stratigraphic level of origin of the grave. But in rare instances the chance association of a human skeleton with the bones of Pleistocene animals may occur, and it may be difficult either to prove or disprove contemporaneity of the human and non-human bones. The fluorine method may solve just such doubtful instances of synchronous association as these.[3] It is unfortunate that presumed stratigraphic associations of this sort must always be plagued by the possibility that the human artifacts or bones may have been intrusive into the lower and more ancient deposits which produce the remains of extinct animals. But in those instances where sufficient time has elapsed since the animal remains were deposited, say since Late Pleistocene times, the fluorine test should prove sufficiently specific to either prove, or indicate strongly, or to disprove outright the question of contemporaneity of the human and animal remains. The age of the human bones may then be inferred with reference to that of the animal bones, as was done by Oakley in the case of the Galley Hill and Piltdown remains from England.

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[3] A. D. Krieger has called our attention to an early application of Carnot's method cited in Luis Aveleyra Arroyo de Anda's *Prehistoria de Mexico* (1950, pp. 32–33), referring to a paper by Herrera ('93, p. 18) in which the "Xico mandible" found in a quarry on the border of Lake Chalco was compared with the skull of a fossil horse *(Equus excelsus* Leidy) found near by with reference to fluorine. Both were practically identical in fluorine level, and Herrera concluded the human mandible and extinct horse remains were contemporaneous. More recently T. D. Stewart has reported in *Science* (April 6, 1951) on fluorine analysis of the Natchez pelvis.

The separate investigation of bone fossilization by Cook and Heizer has indicated that there are several constituents found in fossil bone, the quantities of which alter with time after burial. These included calcium, phosphate, acid extractable carbonate, water, nitrogen and organic carbon. The shift in concentration of all of these does not necessarily follow a consistent secular trend. Indeed we have shown that the inorganic components may be determined simply by the chemical constitution of the soil or matrix within which the bone is buried, although the organic components and water tend almost invariably to diminish with the age of the osseous remains independently of the chemical nature of the matrix in which the bones lay.

The problem of actual temporal decomposition may be omitted from specific consideration here. The point we wish to emphasize is that the argument used effectively by Oakley with respect to fluorine applies with equal cogency to other substances. This argument may be thus expressed: any substance in bone which undergoes a consistent change during interment may be employed for comparing the relative ages of two bones, both of which are known to have been exposed to the identical physical and chemical environment subsequent to burial. The two bones may be human, both animal, or one human and one animal.

This principle greatly enlarges the scope of the chemical method for it enables us to compare samples by means of several criteria simultaneously rather than by one criterion alone. Experimental and other sources of error are thus much reduced. The investigation here reported consists of an attempt to apply these several criteria to a critical series of bone samples.

The components determined in the aggregate are those mentioned above, although for various reasons not all of the components were analyzed in all the series. Included in at least some of the series were fluorine, calcium, phosphate, carbonate, nitrogen, organic carbon and water. The fluorine was determined by the widely employed method of Willard and Winter ('33). The analyses for the other substances were carried out by methods described by us in the papers previously cited. All results are expressed as per cent of the original bone weight.

The present investigation concerns 5 archaeological sites from which have been derived, in apparent association, the bones of humans and various Pleistocene extinct mammals. In each instance there have been claims or intimations, and counterclaims or doubts expressed that the animal and human bones were contemporaneous, notwithstanding the fact or strong presumption of stratigraphic association. The sites are:

1. 4-Cal-10,[4] a limestone cave near Angels Camp in Calaveras County, California.
2. 4-Eld-16, a fissure cave near Auburn, El Dorado County, California, and known as Hawver Cave.
3. 4-Fre-48, an open site near Tranquillity, Fresno County, California.
4. 4-LAn-172, the site of the recovery of a mineralized human skeleton in the city of Los Angeles, Los Angeles County, California.
5. 9-Br-44, the Melbourne site in Brevard County, Florida.

Site 4-Cal-10. This small cave in a limestone area was first discovered during quarrying operations by the Calaveras Cement Company. From time to time as the face of the quarry was broken back, a fissure, floored with the Sierran feruginous red soil and containing bones, was seen. On several occasions Professor C. L. Camp of the Paleontology Department, University of California, visited the site and recovered some fossilized human and animal bones. Because the blasting operations had disturbed the stratification, it was not possible to state categorically that the several mineralized human bones extracted together with those of an extinct *Equus* were originally buried at the same time. The problem of contemporaneity was, therefore, one which the fluorine method might solve.

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[4] California sites are prefixed with the number 4, and site designations are those of the University of California Archaeological Survey. The prefix 9 indicates Florida, and the county and site designation is taken from Rouse (1951, pp. 114, 153–162).

Two human bones (ulna and femur) and two horse bones were analyzed. The fluorine content of the latter is from two to three times as great as that of the human bones, thus indicating (according to the process of slow absorption of fluorine in bone) that the horse bones are much older than those of the humans. At the same time it will be noted (table 1) that the nitrogen and organic carbon content of the human bones are, respectively, approximately 3.6 and 10.5% of the bone weight. These values are relatively high for fossil bone, and correspond to those we have noted from human burials of more recent origin. (Cf. Heizer and Cook, '49, tables; Cook, '51a, tables.) The water content is also of the order of magnitude to be expected in fairly recent bone. On the other hand, the nitrogen, organic carbon and water values for the horse bones are extremely low, and are *per se* clear indication of very considerable age. These wide discrepancies can lead to but one conclusion, viz., that the horse bones are of high antiquity (probably late Pleistocene) and the human bones are quite recent, probably not more than a few hundred years old. There can, therefore, be no temporal equivalence between the human and animal bones from this deposit.

Site 4-Eld-16. This site, a fissure cave near Auburn, El Dorado County, California, was first explored by J. C. Hawver, J. C. Merriam and E. L. Furlong in 1907. A human burial was found in 1908 at the entrance of the lower cave buried under 13 feet of earth and rock. Numerous extinct mammalian remains were found, but there was no direct association of these with the human bones. A number of flaked

TABLE 1

Chemical constituents of fossil bone (expressed in per cent)

SAMPLE	FLUORINE	CALCIUM	PHOSPHORUS	CARBONATE	CARBON	NITROGEN	WATER
4-Cal-10							
Human							
1	0.01	...	...	4.13	10.96	3.37	8.24
2	0.01	...	...	4.21	10.10	3.86	8.21
Mean	*0.01*	...	...	*4.17*	*10.53*	*3.62*	*8.22*
Horse							
1	0.222	...	...	6.66	0.60	0.007	4.38
2	0.300	...	...	8.40	0.10	0.009	4.01
Mean	*0.261*	...	...	*7.53*	*0.35*	*0.008*	*4.19*
4-Eld-16							
Human							
2208	....	31.3	11.4	5.55	6.75	3.35	8.75
2209A	....	38.9	11.9	10.66	2.48	1.26	6.41
2209B	....	38.9	12.6	8.75	2.26	1.19	6.25
2209C	....	35.4	12.5	7.34	2.45	1.63	8.51
2209D	....	34.4	12.1	5.62	3.93	2.27	7.68
2209E	....	41.3	14.1	9.71	1.21	0.39	5.62
2210A	....	39.9	15.1	5.41	3.48	1.30	4.56
2210B	....	41.2	14.6	7.00	1.84	0.88	3.76
2211A	....	38.0	12.1	6.82	4.03	1.75	6.02
2211B	....	40.8	14.0	7.66	1.90	1.08	3.52
Mean	....	*38.0*	*13.0*	*7.45*	*3.03*	*1.51*	*6.11*
Sloth							
11037	....	41.5	10.4	16.69	0.66	0.01	3.91
19888	....	43.3	8.6	24.20	0.19	0.05	4.72
19893	....	43.6	9.6	25.35	0.00	0.02	3.78
11062	....	42.5	15.3	6.55	0.26	0.11	5.21
19967	....	42.7	15.4	6.11	0.58	0.04	7.50
Mean	....	*42.7*	*11.8*	*15.78*	*0.38*	*0.05*	*5.02*
4-Fre-48							
Human							
6071	0.185	...	...	6.78	1.85	0.092	2.33
6072A	0.156	..	...	6.20	1.72	0.110	2.68
6072B	0.102	...	...	6.40	1.85	0.072	2.49
6073	0.208	...	...	6.25	1.62	0.060	1.81
6075	0.136	...	...	6.12	0.73	0.053	1.99
Mean	*0.157*	...	...	*6.35*	*1.55*	*0.077*	*2.26*
Mammal							
51570	0.228	...	...	4.30	3.86	0.060	2.06
61635	0.210	...	...	5.70	2.36	0.053	1.63
61979	0.236	...	...	6.35	2.52	0.070	1.76
64948	0.128	...	...	6.74	1.46	0.019	1.47
Mean	*0.200*	...	...	*5.77*	*2.55*	*0.051*	*1.73*
4-LAn-172							
Human							
1	0.118	...	...	23.10	1.72	0.032	4.60
2	0.044	...	...	33.10	1.77	0.017	2.62
Mean	*0.081*	...	...	*28.10*	*1.75*	*0.025*	*3.61*
Elephant							
1	0.079	...	...	14.20	2.99	0.049	4.50
2	—.152	...	...	16.15	3.46	0.043	4.20
Mean	*0.115*	...	...	*15.20*	*3.23*	*0.047*	*4.35*
9-Bre-44							
Human	0.096	...	...	4.21	2.79	0.052	5.50
Horse	0.119	...	...	3.85	3.43	0.075	6.20
Mammoth	0.116	...	...	2.57	1.97	0.047	3.45

projectile points were recovered from the cave, but their stratigraphic position and associations seem not to have been recorded. The chief publications concerning Hawver cave are listed by Heizer ('48, pp. 11–12) and Hay ('47, pp. 222–223).

The analytical results of the chemical investigation of 10 samples of human bone and 5 samples of sloth *(Nothrotherium)* show a rather drastic variation between the analyses for individual bones (table 1). To explain these we should have to know the details of location and stratigraphy of the separate samples. These details are lacking. Examining the 10 human and 5 sloth samples as two discrete units, we find a distinct difference between them. The sloth bones appear to be somewhat more highly mineralized than the human specimens as indicated by the slightly higher calcium and lower phosphorus content of the sloth bones. On the other hand the extreme variation in carbonate found in the sloth samples suggests that the difference in inorganic components may be merely a reflection of the varying local conditions within the cave operating on individual bones. If this is the case, there can be no significant distinction between the series with respect to these components. The water content is slightly greater in the human bones, and this would imply that the sloth bones are a little older (cf. Cook, '51a). The really significant deviation lies in the contained organic carbon and nitrogen. These elements are to be found in smaller amounts in the human bones from Hawver Cave than is characteristic of late prehistoric skeletal material from Sacramento Valley sites (Heizer and Cook, '49). Thus, one may compare the 3.03 mean per cent of carbon and 1.51 mean per cent of nitrogen found in the Hawver Cave human bones with the corresponding values of 10.53 and 3.62% in those from the Calaveras cave (site 4-Cal-10 described above). From this comparison one may deduce that the Hawver Cave material is moderately to quite old, and not recent in the sense that we so categorized the human remains from 4-Cal-10. The sloth bones contain only 0.38 mean per cent of carbon and 0.05 per cent of nitrogen — a very significantly less amount than the human bones. The organic material has been further depleted in the sloth than in the human bone. The sum of the evidence indicates that whereas the human bones, as a whole, are to be considered as relatively old, they are definitely younger than those of the sloth. The human bones, therefore, were interred at a later time than those of the sloth. It is unfortunate that fluorine determinations were not made, but it may be supposed on the basis of other components that this element would be found in significantly lesser amounts in the human than in the sloth bones from Hawver Cave.

Site 4-Fre-48. The Tranquillity site is well described by G. W. Hewes ('41, '43, '46). Briefly the problem is that 5 human burials, all highly mineralized, were recovered in probable, but not certain, association with chipped and polished stone artifacts, shell beads, bone implements, and unmodified bones of three extinct mammals, *Equus, Camelops* and *Bison.* In Hewes' latest paper, he proposes two alternative interpretations of the finds. First, the remains of the extinct mammals may be older than the human skeletons, and the association of these, together with artifacts may be accidental and the extreme mineralization of the human and extinct animal bones is ''irrelevant.'' Second, he suggests that the earliest occupation by man may have been contemporary with the *Camelops, Bison* and *Equus* assemblage,[5] with other artifacts and burials being later than some of the earliest mammal material. Our chemical tests, presented in table 1, examine the issue of ''irrelevance'' of the mineralization factor. The mammal bones (horse and camel) contain slightly more fluorine and carbon, slightly less water, and approximately (within the experimental error) the same amount of nitrogen as the human bones. In terms of the separate criteria, the fluorine and water would indicate the animal bones as slightly older, the carbon would point to the animal bones as slightly younger, and the nitrogen would place them as equivalent in time. However, considering the relatively small number of samples and their inherent variability these deviations must be regarded as random, and the two lots of bones must be considered as practically identical in chemical constitution. The conclusion follows that they are all of approximately the same age, and that the Tranquillity human bones are of Late Pleistocene age. If the differences expressed between the two bone lots are considered significant and not due to experimental error, the human bones would be slightly later, and therefore be immediately post-Pleistocene. The word ''Pleistocene'' is used loosely here in the sense that the date of mammalian extinction marks its termination. C. L. Camp and R. A. Stirton assure us that the paleontologists are also in the dark as to when this event occurred.

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[5] This assemblage is typically Late Pleistocene or what Savage ('51) calls ''Rancholabrean,'' for which see also Stock ('46).

Site 4-LAn-172. The ''Los Angeles Man'' site has been discussed by several people (Bowden and Lopatin, '36; Clements, '38; Lopatin, '40; Heizer, '48). Human bones, apparently from an articulated skeleton, came from a depth of 13 feet in gravels. About 1000 feet distant from the human remains and lying in the same stratum were found remains of the imperial elephant *(Archidiskodon imperator* Leidy). Clements, a qualified geologist, describes the stratification (7 feet of dark surface soil, two feet of peat, two feet of black clay, one foot of yellow clay and two feet of gravel in which the bones lay). G. W. Hewes supplied two samples of each of the human and elephant remains and the quantitative determinations are shown in table 1. The fluorine content suggests that the elephant bones may be slightly older, but in view of the internal variation and the small number of samples there can be no statistical significance to the difference between the means of .081 and .115%, so that we must conclude that the fluorine determination indicates all of the bones as of approximately the same age. On the basis of the carbon, nitrogen and water content the human material is distinctly older. Again, however, none of these sets of values have statistical significance in their differences, and it would be over-optimistic to claim that the human remains are actually older than those of the imperial elephant. On the other hand, the data seem unequivocal in demonstrating at least an equal age for the human and elephant providing the chemical methods of bone analysis are acceptable. A re-examination of the locality, and securing of a peat sample for radio-carbon analysis is indicated as one means of verifying or disproving our results derived from bone chemistry. *Archidiskodon* is usually attributed to the earlier Pleistocene, and if this be correct, we apparently have a human skeleton which is *too* ancient (see Hay, '26; Gross, '51; Gidley, '29). One may inquire whether the fossil is actually *Archidiskodon* or a later Pleistocene form, and we suggest that this matter be checked by some paleontologist.

Site 9-Br-44. The well-known discovery by J. W. Gidley of human remains at Melbourne, Florida in association with extinct mammals offers us another problem to be tested by our chemical method. The older literature is cited by Sellards ('40, pp. 381–382), and a re-examination of the skull has recently been published by Stewart ('46). Most recently Rouse ('50; '51, pp. 65–67, 153–162) has fully reviewed the evidence of the Melbourne find as an integral part of his Indian River synthesis, and has concluded that the Melbourne skeletal remains probably date from the Preceramic or Orange cultural periods of the Melbourne-Van Valkenburg geologic interval for which he suggests dates between 2000 B.C. and 1 A.D. The Melbourne bone samples, supplied us by Dr. T. D. Stewart of the U. S. National Museum, consist of part of the centrum of a mammoth vertebra, a horse tarsal, and a human tibia fragment. The human bone is stated by Gidley to have come from just below the upper contact place of the No. 2 layer (Melbourne formation). Table 1 shows the results of the determinations. The human sample is slightly lower than the mammoth and horse bone in fluorine, and is intermediate between the horse and mammoth in carbon, nitrogen, and water. The data, so far as they go, are quite consistent and it thus appears that the human sample is of the same order of antiquity as those of the extinct mammals. Either the Melbourne human is actually Pleistocene, or our method of inferring age differences on the basis of fluorine, carbon, nitrogen, water, calcium and phosphorus gives erroneous results.

We feel that our method has much to commend it, but at the same time we hesitate to state categorically that it has demonstrated the practical contemporaneity of the extinct mammals and humans at the Tranquillity, Los Angeles and Melbourne sites. On the basis of a limited number of samples, small differences may be due to either the experimental error, or to actual time differences as reflected in variable amounts of fluorine. Where the radio-carbon dating method can state the amount of error in plus or minus years, the fluorine method does not. This is why the fluorine method yields relative and not absolute dating and why, for even the crude sort of relative dating extractable, the method gives the best results when there are large time differences between two lots of bones.

T. D. Stewart ('50, p. 100) in discussing the implications of the fluorine dating of the Piltdown and Galley Hill remains speaks of the "subjective pit-falls that surround chemical dating" and observes that "Today most everyone accepts at once and without question the findings in an exact science like chemistry. Anthropologists are only human, therefore, when they extend this acceptance to chemical experiments set in the framework of the less exact biological and social sciences. In short my reservations [concerning the fluorine tests of Galley Hill and Piltdown] stem not from the chemical side but from the biological and geological side; from those elements of the experiment that require personal evaluations." We are all anxious not to be too subjective, I am sure, but there are few problems whose solution does not contain the ingredient of personal judgment. It has seemed to us that the general validity of the fluorine method is strongly indicated by the results achieved by Carnot and Oakley, and that the method was worth applying to some of the problematical instances of ancient man in North America. The Melbourne find (like that of Vero, or Minnesota, or Tepexpan) is still a problem precisely because qualified authorities could not agree — in other words, because of the subjective factor. Alternative opinions, each of them forcefully argued and supported by certain salient facts, characterize the status of many important problems of North American prehistory. How can we reconcile Rouse's opinion on the age of the Melbourne human remains with those of Gidley and ourselves? One or the other (or a third hypothesis to be framed at some future date) is correct, but as of this moment which is more probably true cannot be said. We present our results and our opinions as to what they mean, but at the same time do not overlook the existence of alternative opinions based upon other evidence interpreted by different techniques.

We conclude that the method of chemical analysis, utilizing fluorine, organic carbon, nitrogen and water appears to provide justification for distinguishing the relative ages of buried bones from humans and extinct animals, provided such bones are derived from the same stratigraphic horizon. Granted this, it is indicated that man was present in California and Florida prior to the final extinction of some elements of the Pleistocene fauna.

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Pleiotropic Syndrome in San Blas Cuna.— The colorful Caribe-Cuna Indians with their golden nose-rings and interesting devil appeasement ceremonies, have warded off civilization rather successfully for 450 years and they have also battled with a problem of eugenics for many centuries. In the latter battle they are losing ground.

The eugenic problem of the Caribe-Cuna is presented by the fact that they have a high incidence of albinistic Moon-Children. . . .

Measurements of height and weight show that the Moon-Children are slightly shorter and lighter than are normal Cuna Indians. Head shape of Moon-Children appears to be brachycephalic in a higher percentage than normal, but this has not been proven. The facial angle tends to be depressed. Hands are often small and soft. . . .

We have been unable to determine basal metabolisms, but the indications are that metabolic rates of Moon-Children are reduced. Hands are often cold and clammy. Some specific gravities have been taken on urine samples and these are very low. Ultraviolet spectrum analysis of urine samples from boys show some substance, possibly a steroid esterone, to be reduced in quantity, or missing entirely. The Moon-Children do not appear to have an incidence of lung infection above normal, but they constantly carry colds of the nose, head and throat. Readings were taken with a manual dynamometer which show that Moon-Children are reduced in strength in both males and females. They have a slow gait and slow movements. . . .

Moon-Children are slow in developing sexually. The voices of Moon-Children tend to be soft, and this is particularly noticeable in adult males. . . .

Thus, the Moon-Child represents not merely a pigment variation, but rather a pleiotropic syndrome affecting morphology, physiology and psychosomatic behavior. . . .

The Moon-Child variation is due to a mutant gene in duplex, and . . . this same gene in simplex frequency produces a pigmentation lighter than that of the average Indian.— Clyde E. Keeler. An attempt to eliminate a genetic syndrome in man. Eugen. News, vol. 35, no. 3, 1950, pp. 40–44.

PALEOPATHOLOGICAL EVIDENCE SUGGESTING PRE-COLUMBIAN TUBERCULOSIS IN NEW YORK STATE

WILLIAM A. RITCHIE

New York State Science Service and State Museum, Albany

ELEVEN FIGURES

The material of this paper consists of three kyphotic Indian spines from three separate prehistoric and pre-Columbian cultures in New York State. The nature of the bony lesions exhibited in all cases is such as to suggest strongly an etiology of Pott's disease or tuberculosis of the spine. In order either to verify or disprove this assumption the actual specimens, or photographs and radiographs of them, were submitted to 17 specialists, comprising pathologists, orthopedists and radiologists in various parts of the country, all selected for their known interest in tuberculosis or in paleopathology. In many instances these authorities were staff members of large research institutions and medical centers and their statements represent not single opinions but the composite views of numerous colleagues in various special fields of medicine.[1]

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[1] The research for this paper was conducted in 1947 and 1948 with the much appreciated aid of the following doctors: Erwin H. Ackerknecht, Professor of the History of Medicine, University of Wisconsin Medical School; James F. Brailsford, Professor of Radiology, University of Birmingham, England; William L. Brosius, Pathologist, Herman Kiefer Hospital, Detroit; Mather Cleveland, Orthopedist, St. Luke's Hospital, New York; Charles W. Goff, Orthopedist, Hartford, Conn.; Louis A. Goldstein, Orthopedist, Rochester, N. Y.; William B. Hawkins, Associate Professor of Pathology, University of Rochester (N. Y.), School of Medicine; Heywood H. Hopkins, Orthopedist, Rochester, N. Y.; George B. Mider, Associate in Surgery, University of Rochester (N. Y.), School of Medicine; John Morton, Professor of Surgery, University of Rochester (N. Y.), School of Medicine; Walter G. Putschar, Pathologist, Charleston, W. Va.; George H. Ramsey, Clinical Professor of Radiology, University of Rochester (N. Y.), School of Medicine; T. Dale Stewart, Physical Anthropologist, U. S. National Museum; Hans Smetana, Army Institute of Pathology, Washington, D. C.; Alan De Forest Smith, Director and Attending Surgeon, New York Orthopaedic Dispensary and Hospital; P. F. Titterington, Radiologist, St. Louis, Mo.; George H. Whipple, Dean, University of Rochester (N. Y.), School of Medicine.

MATERIAL

1. AP 526 is the complete skeleton of an adult female, between 26 and 30 years of age, found during the course of excavations in 1938 in a small cemetery of flexed burials on the Durkee farm, situated on the Genesee River, one and a quarter miles north of Avon, Livingston County, New York.[2] It pertains without doubt to the prehistoric Iroquois occupation of this region. The Seneca tribe of the Confederated Five Nations of the Iroquois were the final aboriginal occupants of this region, which was in their possession until after the American Revolution. According to current archaeological evidence the ancestral Seneca were present here about A.D. 1,200. They became, before the historic period, a populous village-dwelling people, inhabiting communal bark longhouses and primarily dependent upon crop-raising (corn, beans, squashes) for subsistence. In the early prehistoric period, to which the Durkee cemetery pertains, the population was smaller and more scattered, and the communities consisted of hamlets rather than villages.

The lower 7 thoracic and all of the lumbar vertebrae of this specimen, shown in figures 1, 2, were involved in a destructive process which has resulted in the almost complete absence of one vertebral body and the loss of most of another. Most of the necrosis has occurred in the anterior portion of the bodies producing a collapse of the spine at an approximate 90° angulation. Two areas of partial or complete fusion of vertebral elements occur, one in the lumbar region resulting in some degree of compensatory scoliosis or lateral angulation. Multiple cloacae or drainage canals involving the bodies, pedicles, and articular facets indicate a disease process of long standing. The erosion of the anterior body surfaces is indicative of a large perivertebral abscess which apparently enveloped the entire dorsal column beneath the periosteum and ligaments. Although the process was essentially destructive considerable healing has taken place with attendant fusion and articular ankylosis.

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[2] The archaelogical excavations which produced specimens AP 526 and AP 529 were conducted by Charles F. Wray and the writer who was then Archaeologist of the Rochester Museum of Arts and Sciences. The three specimens described are in the collections of that institution and are used with permission. The numbers are catalogue designations.

To 5 of the examining experts this specimen presented an unequivocal picture of healed tuberculosis. Eight others regarded tuberculosis as the most probable etiological factor. Two interpreted the lesions as suggestive of actinomycosis or some other mycotic infection, but did not rule out tuberculosis. One concluded in favor of an extensive old osteomyelitis, while another also believed that pyogenic infection was the probable agent. Thus the general consensus supported a diagnosis of Pott's disease.

2. AP 529 is the entire skeleton of an adult male approximately 40 years of age, found with other typically flexed burials in the excavation of a small cemetery on the Peter Rapp farm, on Honeoye Creek, at Golah, near West Rush, Monroe County, New York, in 1939. Some of the burials were accompanied by grave goods characteristic of the Canandaigua Focus, Owasco Aspect, Woodland Pattern, a late prehistoric and pre-Iroquoian culture in the New York area.

The Owasco culture, known from numerous excavations, resembled the Iroquois culture in basic economy. Its authors were also farmers, hunters and fishermen, who established progressively larger settlements, many of them fortified with palisade lines. While no radiocarbon dates have yet been determined for the Owasco sequence, its recognized developmental stages, of which the Canandaigua Focus is near the bottom, are estimated to span some 7 centuries, beginning approximately A.D. 500 (Ritchie, '44, '51).

This specimen, illustrated in figures 4–7, shows a profound lesion which has destroyed the centra of the vertebral bodies of most of the thoracic segments of the spine, producing a kyphosis of slightly less than 90°. The remnants, together with the lumbar vertebral bodies, have undergone firm fusion. A marked atrophy of the anterior lumbar bodies may have resulted from the pressure of a thick walled-off abscess in the abdomen. Sinus formation and a slight amount of regenerated bone are also present. The total picture is one of a chronic, low-grade, inflammatory process during which the patient was ambulatory, and according to 10 of the authorities consulted, it is most consistent with a diagnosis of Pott's disease. One pathologist inclined to regard it as a partially healed osteomyelitis, possibly following wounds with bacterial or mycotic infection, while two found the specimen very difficult to evaluate and rendered no opinion. Here again the majority opinion favored an etiology of tuberculosis.

3. AP 530. Unfortunately this specimen is all that remains of an apparently young adult skeleton which was buried with grave goods diagnostic of the middle Point Peninsula culture (Focus 3), on Kipp Island in the Seneca River, Seneca County, New York. It was one of many found in 1938 by relic hunters who retained few of the skeletal remains. The obviously extraordinary character of this spinal fragment led to its preservation and subsequent presentation to the Rochester Museum of Arts and Sciences. I have carefully verified the statements of the finder, studied the site and collections from it, and unequivocally accept the pre-Columbian age of the skeleton (Ritchie, '44).

The specimen, various aspects of which are photographed in figures 8–11, consists of 7 thoracic vertebrae, ankylosed into a solid piece which exhibits a complete kypho-scoliosis. The neural arch structures, both facets and laminae, are fused to the degree that they have completely lost their identity. There is no evidence of reactive bone changes in these areas. The spinous and transverse processes are intact except for postmortem destruction, and the vertebral canal is patent throughout, indicating that the spinal cord was not encroached upon. Fine bony trabeculations extend throughout the fused bodies, indicating either original absence of separation or more probably excellent healing with new bone bridging the defect to produce a dense ankylosis.

In evaluating this remarkable specimen two of the experts have suggested trauma as the provocative agent, but several others have pointed out that an amount of trauma sufficient to produce it would probably have carried with it lethal shock. Congenital anomaly has also been invoked by two pathologists, but others have noted that a congenital lesion involving the vertebral bodies seldom if ever exists alone. Segmental irregularities usually affect all elements of that particular segment. The likelihood of pyogenic osteomyelitis, suggested by one pathologist, seems vitiated by the statement of others that such marked deformity is not seen in pyogenic osteomyelitis of the vertebrae. Eleven specialists see in this specimen the ravages of long-standing, low-grade tuberculosis.

DISCUSSION

The general consensus therefore supports the view that these three kyphotic spines, from as many cultures in New York State, probably testify to the occurrence in this area of tuberculosis in pre-Columbian times, insofar as can be determined from dry bone pathology. It is interesting to note in this connection the occurrence on various New York Iroquoian Indian sites of effigy pottery pipes, such as illustrated in figure 3, depicting hunchbacked individuals with pigeon breast deformation and the characteristic facies of tuberculosis.

Clay figurines and effigy water bottles of the Middle Mississippi horizon in Tennessee, Arkansas, Missouri and elsewhere in the southeastern United States, are likewise faithful representations of persons afflicted with Pott's disease, while Peruvian vessels of the Mochica period simulating the typical gibbous lesion of the disease are known in some numbers.

Webb ('36) and Requena ('46) have described other art objects from Peru, Mexico, the American Southwest and elsewhere, revealing what the former regards as "the unmistakable picture of Pott's disease." He concludes that "it is almost inconceivable that the American Indians could imagine such a picture."

Kidder found at Pecos, New Mexico, the skeleton of a middle-aged female of the Glaze IV (pre-Spanish) period, bearing an arthritic lesion of the right shoulder and the ravages of possible Pott's disease (Hooton, '30); García Frías ('40) reported on three probably prehistoric kyphotic mummies from Peru to which he assigned a tuberculous etiology; while Requena ('46), on the basis of three carious dorsal vertebrae found by him in 1945 in a precontact archaeological context at El Palito, State of Carabobo, Venezuela, arrived at a differential diagnosis of vertebral tuberculosis.

The writer has recently seen, through the courtesy of Dr. P. F. Titterington of St. Louis and Dr. Don Dickson of Lewiston, Illinois, a specimen very similar to AP 530, found in the Dickson cemetery in Fulton County, Illinois (Cole and Deuel, '37) which pertains to the Middle Mississippi cultural horizon of pre-Columbian age, and there are doubtless many other unrecorded examples.

Shattuck ('38) opines that "tuberculosis of the bones may have existed among the Indians in pre-Columbian times" and even the extremely conservative Hrdlicka ('09) offers no categorical denial of this possibility.

Since the tubercle bacillus attacks bone much less frequently than soft tissue, the incidence of bony lesions is not necessarily indicative of the frequency of the disease in the population of a prehistoric cemetery. The evidence presented in this paper would seem to heighten the probability that tuberculosis was not an unknown entity among the American Indians prior to European contact.

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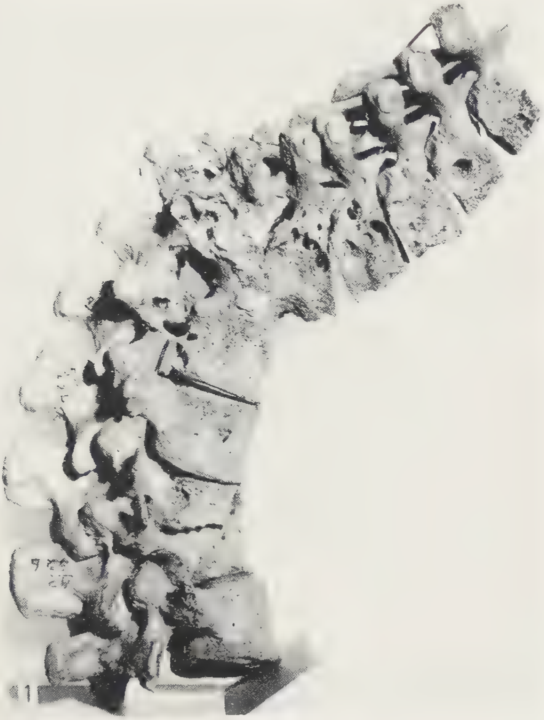
PLATE 1

EXPLANATION OF FIGURES

1 Right lateral view of thoracico-lumbar section of AP 526.

2 Left ventro-lateral view of same specimen.

3 Pottery pipe of Cayuga Iroquois culture from Cayuga Co., N. Y., showing kyphosis coupled with pigeon breast deformity and facies of tuberculosis. Reproduced with permission of Maynard A. Cramer, Auburn, N. Y.



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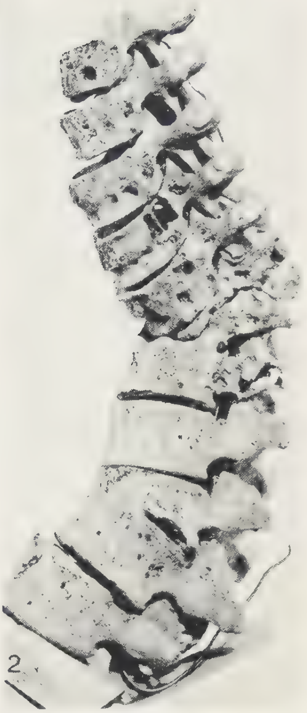

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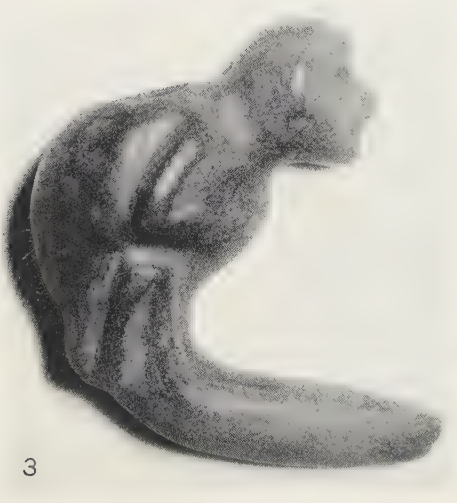

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PLATE 2

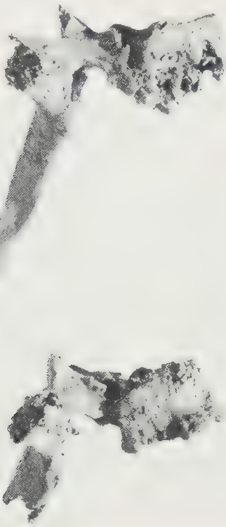
EXPLANATION OF FIGURES

4 Right lateral views of upper thoracic vertebrae of AP 529 showing extensive destructive lesions of the bodies.

5 Left lateral views of same specimens.

6 Anterior view of fused thoracico-lumbar section of AP 529.

7 Left lateral view of same specimen.



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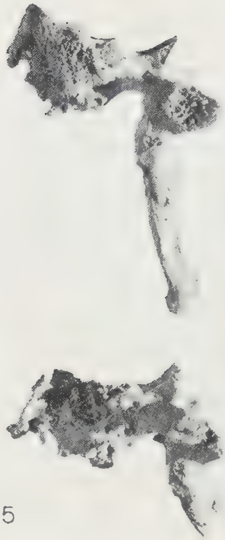

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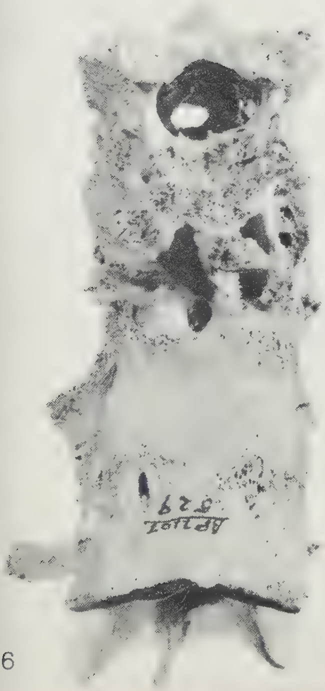

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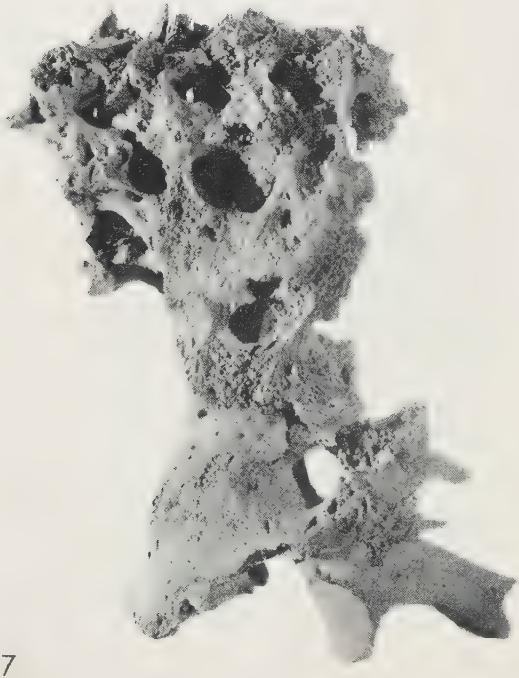

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PLATE 3

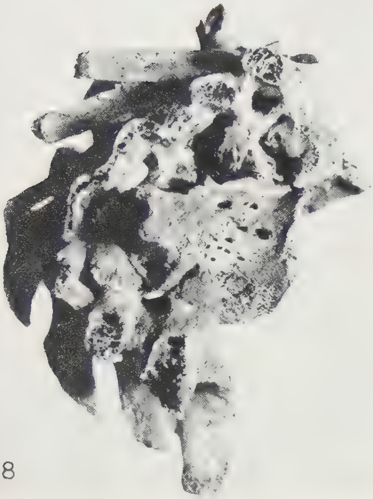
EXPLANATION OF FIGURES

8 Right lateral view of AP 530.

9 Left lateral view of same specimen.

10 Anterior view of same specimen.

11 Left lateral roentgenogram of AP 530 showing bony trabeculations extending through fused bodies.



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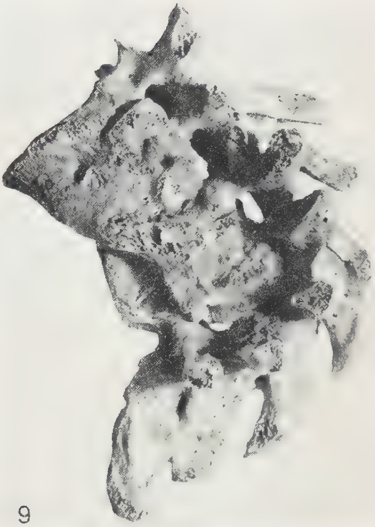

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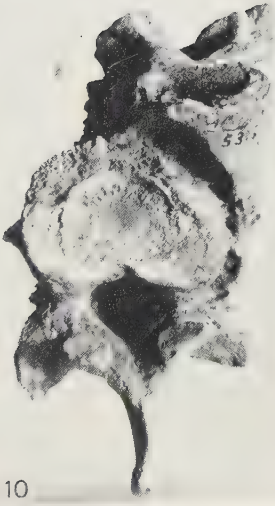

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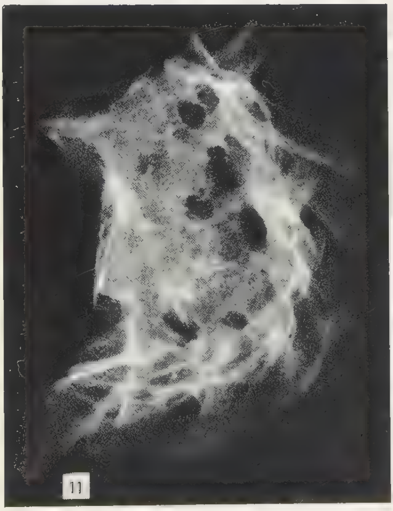

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Genetic Factor in Epilepsy.— A history of seizures was obtained for 3.2% of the 20,000 near relatives of 4,231 epileptic patients. The incidence was 3.6% if evidence of brain damage prior to the patient's first seizure was lacking, and 1.8% if such evidence was present. These percentages are, respectively, 7 and 3.5 times the incidence of epilepsy among draftees for the United States Army.

As additional evidence of a genetic factor, in the group with undamaged brain (essential epilepsy) the incidence of epilepsy among relatives decreased progressively with a later onset of seizures (7.6% if onset was in infancy; 1.5% if it was after the age of 30). Corresponding incidences for the group with brain damage (symptomatic epilepsy) were 2.9 and 1.3%.

The relative influences of heredity, brain damage and chronicity of seizures were studied in a group of 122 twin pairs affected with seizures. In twin pairs without prior brain damage, both co-twins were epileptic in 84% of the one-egg and in 10% of the two-egg twins. In pairs with brain damage, the corresponding incidences were 17 and 8%.

In one-egg twins, concordance was usual not only with respect to seizures, but also with respect to the type of seizure and to the electroencephalographic pattern. . . .

The genetic factor in epilepsy is probably no greater than it is in many other common diseases. The epileptic is unfairly pilloried in public opinion and in law. Assets that are transmissible, such as sound vital organs, good intelligence, personality and social responsibility, may outweigh the liability of a tendency to seizures. Hence advice regarding marriage and children must be individualized.— William G. Lennox. The heredity of epilepsy as told by relatives and twins. J. Am. Med. Assn., vol. 146, no. 6, June 9, 1951, pp. 529–536.

TORUS MANDIBULARIS: ITS OCCURRENCE IN ALEUT CHILDREN AND ITS GENETIC DETERMINANTS

COENRAAD F. A. MOORREES, RICHARD H. OSBORNE
AND EDWIN WILDE[1]
The Forsyth Dental Infirmary, Boston, and Columbia University

FOUR FIGURES

Torus mandibularis is a bony exostosis known to occur in some populations on the lingual side of the mandible near the roots of canine and premolars (see fig. 1). This exostosis was first described by Danielli in 1884, and was named "torus mandibularis" by Fürst ('08). The location is very constant (Fürst-Hansen, '15), but variations do occur as to the extent of the anterior and posterior limits. It is found "most commonly" bilaterally (Schreiner, '35; Akabori, '39), although Lasker ('47) has reported frequent unilateral occurrence in white mono- and bizygotic twins.

Reports in the literature indicate that it can be present in all races of man, but the highest frequency is reported in the yellow-brown groups and in peoples living in the Arctic. The dimensions of torus mandibularis are claimed to increase with age. Its genesis is generally attributed to environmental factors. Hrdlička ('40), in one of the latest and most complete reviews to date dealing with this exostosis, strongly supports the functional hypothesis which contends that torus mandibularis is a response to functional demands. A near-absence of torus in children, an association of torus mandibularis with tooth wear, which can be looked upon as a measure of function

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[1] Formerly physician of the U. S. Fish and Wildlife Service, St. Paul Island, Alaska.

(Kajava, '12), and an increase in its size with age all seemingly support the "functional" hypothesis.

Schreiner ('39) and van den Broek ('41) visualize torus as a localized bone growth due to abnormal sensitivity of the bone conditioned by "deficient diet or avitaminosis," or to "chemical irritation of the mucous membrane." Fürst-Hansen ('15), Middleton Shaw ('31) and Drennan ('37) entertained the possibility that genetic rather than environmental factors were responsible for its causation, but offered no evidence in support of their point of view.

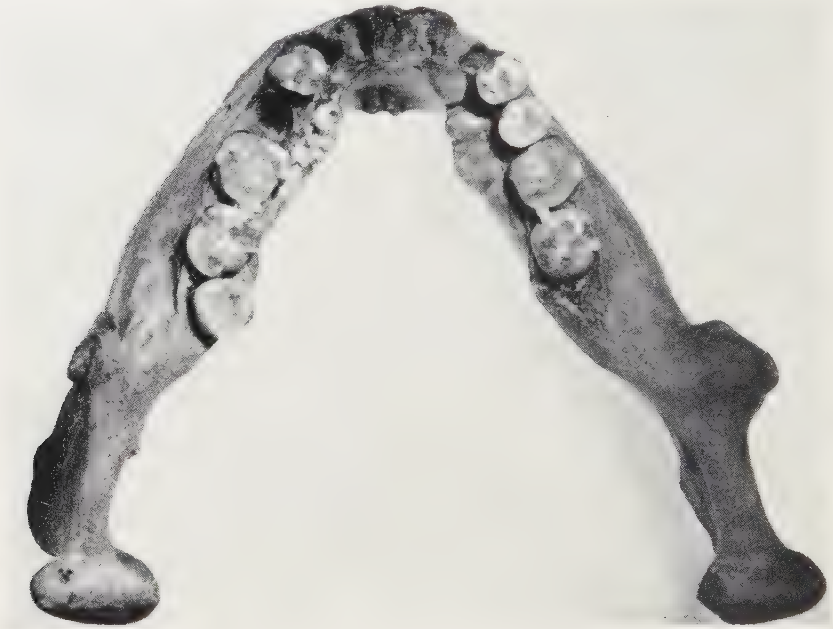

Fig. 1 A mandible showing bilaterally occurring "torus" on the lingual side. (Specimen courtesy Peabody Museum, Harvard University.)

Most previous studies have been done on skeletal material which included only a few children. This scarcity of evidence on immature mandibles may have led earlier writers to conclude that torus is virtually absent in children. Furthermore, skeletal material is not satisfactory, as a rule, for determination of a possible familial incidence and its bearing on the formation of torus mandibularis. The opportunities to obtain more adequate material for the study of torus mandibularis are therefore better in a living population.

The data obtained during a field study of the Aleut [2] living on Atka and Umnak Islands and of Aleut children living on the Pribilof Islands were found to be of value for this purpose, because they provided information on occurrence of torus in children and its familial incidence.

Torus mandibularis was palpated for its presence and graded as trace, slight, marked, or extreme, following a 4-point scale presented by Hooton ('18). Although great care was exercised and a number of double determinations were made, the authors are aware of the shortcomings of this subjective scale of evaluation. With regard to children it may be stated that, if any errors were made, the torus has not been overrated. Torus size was always related to the size of the jaw in order to obtain the same relative position on the 4-point scale.

TABLE 1

Incidence of torus mandibularis in Aleuts of all ages and both sexes

	BOTH SEXES		MALES		FEMALES	
	N	%	*N*	%	*N*	%
Absent	70	64.8	33	57.9	37	72.5
Trace	5	4.6	2	3.5	3	5.9
Slight	14	13.0	9	15.8	5	9.8
Marked	16	14.8	10	17.5	6	11.8
Extreme	3	2.8	3	5.3		
Total	108		57		51	

Torus mandibularis was found to be present to some degree in 35% of the 108 Aleuts living in the Aleutian Islands chain (table 1). When the data are broken down into age groups (fig. 2), a gradual increase in the relative number of the more marked forms with advanced age is evident. Yet in 30% of the youngest group (1–10 years), torus mandibularis was found to occur, and the records show that even a 2.69 year old male infant had a "slight" torus.

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[2] Nineteen hundred and forty-eight, The Peabody Museum, Harvard University, Aleutian Expedition; supported by The Viking Fund Inc. and the Office of Naval Research.

In order to obtain additional evidence on the incidence of torus in children, a study of a closely related Aleut population living on the Pribilof Islands was conducted. A number of adults from Atka and Nikolski, who were employed during the sealing season on the Pribilof Islands, and who had been previously graded by Moorrees for size of torus, were utilized by Wilde for checking the accuracy of his own grading technic in studying the Pribilof Island material. Through this procedure, closer similarity between the two series of evaluations could be expected, with equal caution in the rating of children.

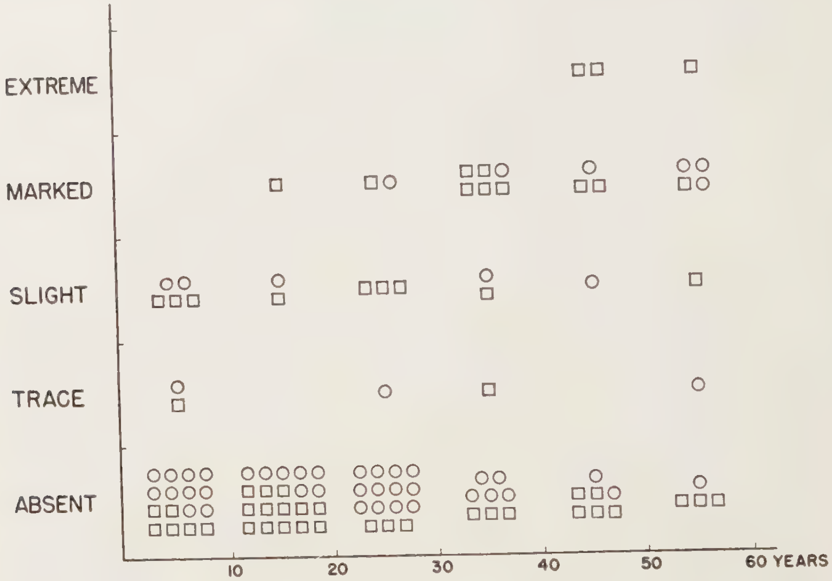

Fig. 2 The incidence of torus mandibularis in Aleuts living in villages on Atka and Umnak Islands. Presence of torus in males and females is indicated by square and round symbols, respectively.

The incidence of torus mandibularis in children from the Pribilof Islands was found to approixmate that of children from Atka and Umnak Islands. The data have been pooled for presentation in table 2.

The incidence of torus mandibularis in the combined series of Aleut children between 1 and 17 years of age is 23.9 ± 3.3%. Sex differences in incidence and degree of development are very small. In order to determine the consistency of the sex differences, a much larger sample is needed. Torus mandibularis does not seem to increase in size in the age span studied (figs. 3 and 4). The data from the Aleuts living on Atka and Umnak Islands suggest that an increase in size occurs toward middle age (fig. 2). These conclusions should be accepted with caution because of the subjective determination of torus development. The principal value of the data presented consists of evidence that torus mandibularis occurs to an appreciable degree in the young.

TABLE 2

Incidence of torus mandibularis in Aleut children from one to 17 years of age

	BOTH SEXES		MALES		FEMALES	
	N	%	*N*	%	*N*	%
Absent	124	76.1	65	75.6	59	76.6
Bilateral						
trace	5	3.1	3	3.5	2	2.6
slight	16	9.8	10	11.6	6	7.8
marked	8	4.9	5	5.8	3	3.9
extreme	1	.6			1	1.3
Unilateral						
trace and slight	9	5.5	3	3.5	6	7.8
Total	163		86		77	

This discovery of torus mandibularis in Aleut children is contrary to most previous findings, but by no means unique. Fürst-Hansen observed torus in quite young Greenland Eskimo children and young individuals. Schreiner reports a high incidence of torus in Lapp children from the villages of Angsnes and Kistrand in Norway (34.8% in children and 48.7% in adults).

The presence of torus mandibularis in children is of interest in relation to the presumed cause of this exostosis. This and other findings — a lesser incidence in females who use their dentition more than the men in chewing sealhides, etc., a preponderance of the more marked degrees of torus in the males, and an absence of any association with tooth wear as a measure of function — all result in evidence which strongly suggests that the presence or absence of torus mandibularis is not a

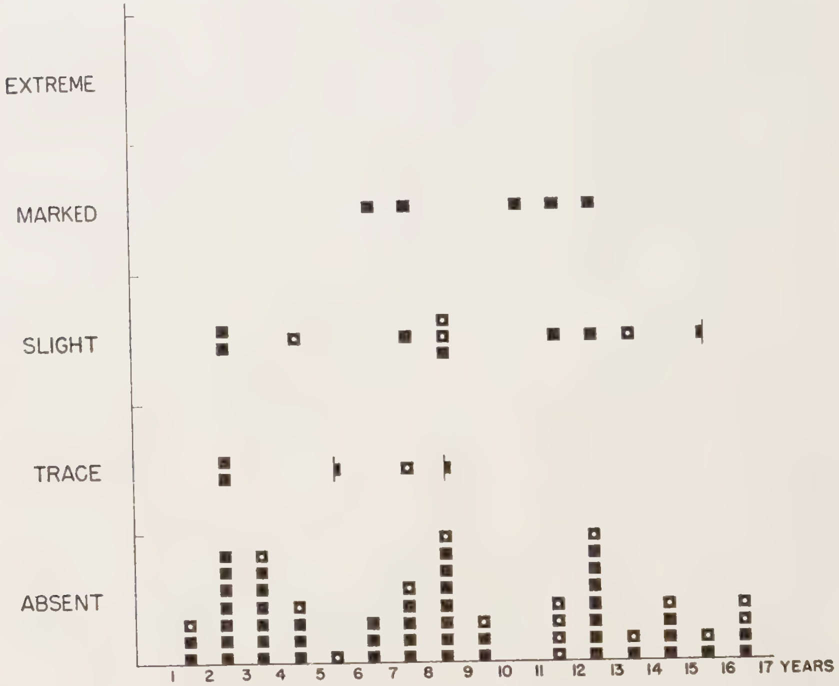

Fig. 3 Raw data on the incidence of torus mandibularis in male Aleut children of various ages living on St. Paul Island (closed symbols) and on Atka or Umnak Islands (open symbols). Each symbol refers to one individual. Unilateral presence of torus is indicated by a vertical line and a halved symbol. The location of this halved symbol either to the left or right side of the vertical line furthermore indicates the side of the mandible where torus was observed.

purely functional response. These findings support indirectly the concept of a hereditary basis for this exostosis.

Furthermore, different frequencies of torus mandibularis in populations having comparable diets and habits of mastication are reported in the literature. A similar observation was made concerning the incidence of torus mandibularis in the population of the Aleutian Islands, which, according to Laughlin ('51), actually consists of two separate breeding isolates, the Eastern and Western Aleut. Laughlin's finding is based on evidence obtained from blood group frequency tests. The Eastern Aleut, who live east of Atka, and the Western Aleut, who inhabit Atka, and formerly Attu, exhibit a difference in incidence of torus mandibularis (C. R. 3.4) and yet are living

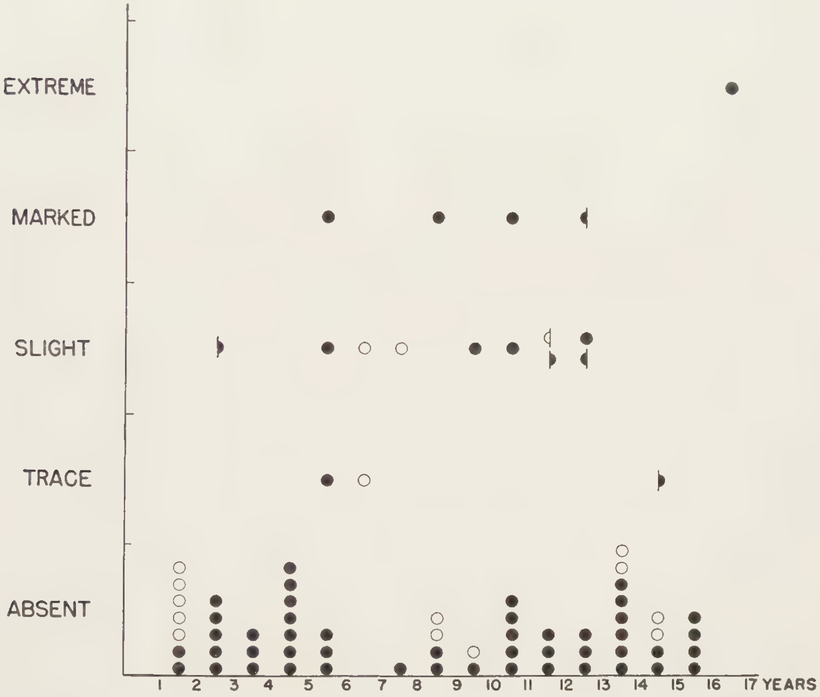

Fig. 4 Raw data on the incidence of torus mandibularis in female Aleut children of various ages. See legend of figure 3 for additional explanation.

in an environment and culture which is identical in nearly every detail (table 3). This difference again focuses attention on genes rather than on the environment. Since the torus appears in a fairly large number of Aleuts of all ages, it is feasible to investigate its possible mode of inheritance in this population. After recording the data on genealogical charts designed by Garn, a familial incidence of torus became evident.

Yet, since the manner of inheritance of most quantitative characteristics is complex, a series as small as the Aleutian population cannot furnish adequate genealogic data for conclusive analyses. The writers feel, however, that even an inconclusive discussion is justified in view of the interest shown by anthropologists and odontologists in torus mandibularis, the prevalent unsatisfactory ideas concerning its genesis, and the relative excellence of the Aleutian material for such a study. This material provides an opportunity to examine the genetic nature of an apparently independent, quantitative, skeletal characteristic, of a type so important in the study of fossil man and human evolution. The implications attached by the physical anthropologist to such characteristics are quite different if the origins are assigned to genetic rather than to functional determinants. If in the present study at least the *minimum* number of genetic factors involved can be deter-

TABLE 3

Geographic differences in incidence of torus mandibularis with all ages and both sexes combined

	WESTERN ALEUTS		EASTERN ALEUTS	
No. of males	17		22	
No. of females	18		22	
	N	%	*N*	%
Absent	26	74.3	17	38.6
Trace	2	5.7	2	4.5
Slight	5	14.3	9	20.5
Marked			15	34.1
Extreme	2	5.7	1	2.3
Total	35		44	

mined, evidence on the apparently decreasing degrees of development and diminishing frequencies in recent populations becomes more meaningful.

The incomplete genealogies and the small number of individuals, make a pooled sib analysis of the Aleutian population impossible. If the 5 arbitrary categories of torus development were taken as genetic classes, the simplest mode of inheritance suggested would be two genes of cumulative effect. This hypothesis fails in a pedigree analysis of two instances where age and sex influences can be ruled out. Therefore, at *least* three independent loci affecting torus development must be assumed to satisfy all cases represented in the pedigrees of the two Aleutian populations, clearly indicating a complex rather than a simple mode of inheritance. However, the data are not sufficient to determine how complex the mode of inheritance may be. The evidence is furthermore insufficient to determine whether a cumulative or major gene modifier mechanism is operating.

There is only a low correlation between parents and offspring in the degree of torus development, yet between siblings the correlation coefficient is + 0.72, which is significantly high. That this high correlation is not due to the close age correspondence of siblings in the presence of the apparent age factor (fig. 2) is indicated by a coefficient of correlation of only + 0.36 for age and degree of torus development.[3] The conclusive evidence against a purely functional genesis for torus development leaves the phenomenon of a high sibling and a low parent-offspring correlation without explanation at the present time. The chance of a fully satisfactory genetic explanation is impossible without more complete genealogies, that would allow an analysis of the path coefficients and thereby an estimate of the relative fraction of Aleut and non-Aleut inheritance of each individual. The expression of a multiple-factor racial characteristic in a recently mixed population is dependent upon the recombination of the chromosomes derived from the appropriate racial stocks. Under certain conditions this could conceivably result in the apparently conflicting correlations found in the Aleutian population.

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[3] The correlation coefficients were calculated from a two way frequency table utilizing the product moment formula. A ''t'' test of these correlations gives significance at less than the 1% level in both instances. Similarly, a test of the difference between the two correlation coefficients gives a p of less than 1%.

The authors believe that the accumulated evidence presented is sufficient for stimulating further study in order to test the hypothesis of the highly probable genetic causation of torus mandibularis and to give a more precise explanation of the mechanism of its inheritance.

The authors wish to express their appreciation to Prof. T. G. Dobzhansky, Dr. V. O. Hurme and Dr. L. D. Sanghvi who read the manuscript and offered many helpful suggestions during the analysis of our data.

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FREQUENCY OF OVERWEIGHT IN AMERICAN POPULATION.— How many adults in this country are overweight? We cannot answer the question exactly because no sharp line divides the overweight from the normal-weight person. Under the circumstances, the most suitable basis for the answer is to take an arbitrary percentage departure from average weight for height. For men and women over 25 a fixed set of standards based on the average weight at the ages of 25 to 30 is recommended, with due allowance in the individual case for those factors in body structure which influence weight. The ideal weight tables now in popular use, which were prepared several year ago by the Metropolitan Life Insurance Company, take these factors into account.

As a practical measure, we may define overweight as any deviation of 10% or more above the ideal weight for the person. On this basis, at least one-fifth of the population over age 30, or about 15 million, are overweight, and a considerable number of younger people weigh more than is good for them. We would consider that a weight 20% or more above the ideal constitutes pathological overweight, or obesity which definitely requires correction. On this basis the number of obese adults is 5 million or more.— Donald B. Armstrong, Louis I. Dublin, George M. Wheatley and Herbert H. Marks. Obesity and its relation to health and disease. J. Am. Med. Assn., vol. 147, no. 11, Nov. 10, 1951, pp. 1007–1014.

RACIAL INCIDENCE OF DUFFY BLOOD GROUP.— Blood samples from 73 pure-bred Brazilian Indians in the interior of the Mato Grosso, Brazil, have been examined by us for the presence of the antigens *A, B, C, c, D, E, e, M, N, S, P,* Lutheran, Kell and Duffy *(Fy^a)*. An account of this work will be published elsewhere in full.

The point of special interest is that all these Brazilian Indian bloods were Duffy-negative.

. . . The most complete published figures, those of Race and Sanger, show that *Fy^a* is present in the blood of 65 per cent of English people. Cutbush and Mollison have found it in 91 per cent of people from northwest Pakistan; Miller, Rosenfield and Vogel in 99 per cent of Chinese and in 26 per cent of American Negroes; and Hartmann and Brendemoen in 97 per cent of Norwegian Lapps. The fact that all Brazilian Indians so far tested should lack this antigen is a fact which may prove to be of considerable anthropological importance.— A. M. Pantin and P. C. Junqueira. Blood groups of Brazilian Indians. Nature, vol. 167, no. 4259, June 16, 1951, p. 998.

PREDICTING BODY SPECIFIC GRAVITY.— By use of the anthropologist's rating of the first component of physique it is possible to predict body specific gravity, the standard error of estimate for the regression equation presented being ± 0.008 specific gravity units. Thus, in estimating body fat from ratings of *component I* one's error will exceed ± 4.3% fat in only one-third of the cases. For the 81 subjects studied the average deviation by prediction of specific gravity from physique was 0.007 from the value obtained by the immersion method, the error of which is 0.004. . . .

. . . A simple rating of the amount of first component in a series like this cannot be expected to give a perfect correlation with specific gravity, since the factor of individual weight change has not been considered.

It seems probable that by study of a larger series a formula could be devised which would improve on the prediction of body specific gravity and percentage of body fat by use of only the first component of the somatotype. . . .— C. W. Dupertuis, G. C. Pitts, E. F. Osserman, W. C. Welham and A. R. Behnke. Relation of specific gravity to body build in a group of healthy men. J. Appl. Physiol., vol. 3, no. 11, May, 1951, pp. 676–680.

PHOTOGRAPHY: AN ANTHROPOMETRIC TOOL

J. A. GAVAN,[1] S. L. WASHBURN AND P. H. LEWIS
Department of Anthropology, University of Chicago

FOUR FIGURES

In problems concerning human genetics, race, growth, and constitution, the descriptive techniques of anthropometry, direct observation and photography have all been used by various investigators. Frequently all three have been used in the same study, but the possibility of developing a standardized, scale photography as a method of minimizing the errors of anthropometry and observation has not been recognized. A scale photograph of technical excellence offers a permanent record and a direct check on many standard measurements and observations. If selected anatomical points are marked on the individual, many measurements can be taken as accurately on the photograph as on the individual himself. Even when a measurement cannot be taken on the photograph, it still maintains a characteristic relation to the anthropometric one. In the case of bigonial diameter, for example, the anthropometric measurement is less than the corresponding diameter on the photograph, which can be used as a check both on the measuring technique and on the recording. Any personal bias in rating browridge size will be apparent in the photograph and proper correction can be made. In short, a large part of the error inherent in traditional systems of measurement and observation can be eliminated by the use of modern photography.[2]

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[1] Now at Yerkes Laboratories of Primate Biology, Inc., Orange Park, Florida.

[2] A generous grant from the Wenner-Gren Foundation for Anthropological Research made it possible for us to pursue our interest in photographic anthropology. In addition, Gavan was awarded a Wenner-Gren Pre-doctoral Fellowship which enabled him to spend considerable time on the project.

The use of photographs as an aid to description is not new. Martin ('28) gives detailed instructions for the taking of photographs, and the various German journals (e.g., Zeitschrift für Morphologie, Zeitschrift für Rassenkunde, Zeitschrift für Konstitutionslehre) make routine use of photographs. This is in contrast to Hrdlička ('39) and Hooton ('46) where no photographic instructions are to be found. The British and American journals, Biometrika, Human Biology or American Journal of Physical Anthropology, contain few, if any, photographs. In fact it was not until Sheldon published his "Varieties of Human Physique" ('40) that Americans began to realize the utility of photography.

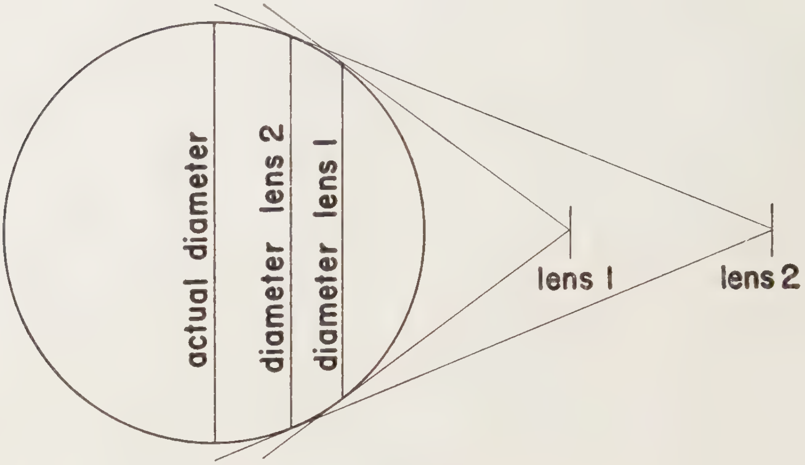
But even where photography has been used most, it has been supplementary to the traditional, anthropometric instruments rather than a full partner in the collection of data. This tradition, that photography and anthropometry are distinct techniques, is clearly seen in constitutional investigations where data of use in non-constitutional problems cannot be obtained from the photographs. Garn and Gertler ('50) have applied both systems to the same series of subjects apparently without recognizing the possible relation between the two. Even though the study of constitution may require special data, it would seem useful to take the photographs so that traditional anthropometric data may be obtained also. The two could then be directly compared. If the photographs are of known scale and meet a few requirements to be outlined later, this becomes a simple matter.

Certainly one reason why photography has not been used more in the past is that anthropologists have rightly doubted the accuracy of the photographic record. Measurements taken on snapshots bear little relation to anthropometric ones; the labor of converting the usual photographic measurements to actual ones is so great that it is far easier to measure directly. The ordinary photograph may be distorted because the paper has shrunk or expanded in an unpredictable manner or because the original lens-subject distance was too short. Further, a slight change in lighting may emphasize or eliminate features. All these sources of confusion can be eliminated provided the total photographic procedure is planned for the specific purpose of producing a standard, scale picture suitable for taking measurements and observations. In this paper we will outline the general requirements of a standardized photography for anthropometric measurements, offer the results of our first efforts, and discuss the implications for anthropology.

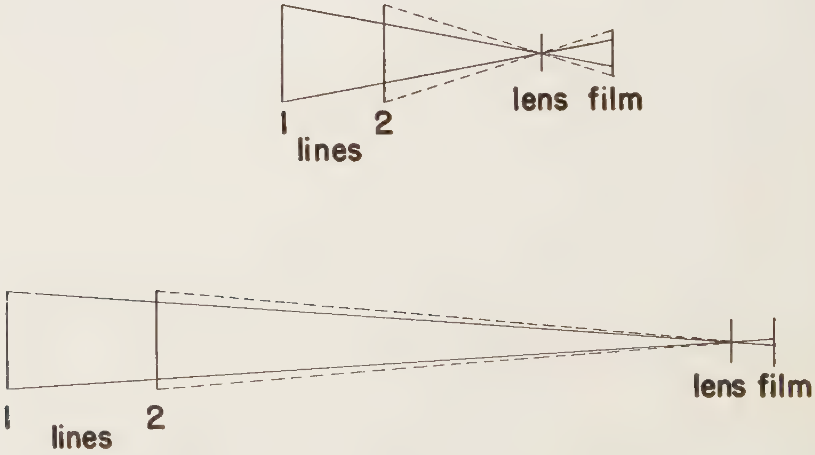
The problem of distortion

Photographs, in order to eliminate the errors inherent in traditional anthropometry, must be free of distortion. Fortunately, the major cause of photographic distortion is a simple one: having the lens too close to the subject. A short lens-subject distance introduces two kinds of errors. First, the photographic contours will be smaller than the actual ones; second, those parts of the subject closer to the lens than the plane on which the camera is focused will be enlarged. The reasons for these two types of distortion are shown in figure 1. In figure 1a a sphere is shown viewed from two distances. The diameter as seen by the nearer lens is smaller than that seen by the further lens; the photographic diameter is always smaller than the true one unless the lens is at infinity. In figure 1b two lines of equal height are viewed by two lenses at different lens-subject distances. The closer lens reproduces the lines as of different lengths whereas the further lens reproduces them as of approximately the same length. Again they will be exactly the same only at infinity. Note that the focal length of the lens has no effect on either type of distortion. Both sources of distortion, the incorrect rendering of the profile and the relative enlargement of nearer parts, are inherent in photography and cannot be entirely eliminated. Both can be reduced to less than the error of measurement by using a long lens-subject distance.

The question of how long a distance should be used cannot be answered dogmatically for it depends on how much distortion is objectionable and on the size of the subject being



A. Incorrect rendering of the profile.



B. Enlargement of parts closer to lens than the plane of focus.

Fig. 1 Two types of photographic distortion. Both are reduced by increasing the lens-subject distance.

photographed. If the subject is posed so that all measurements are taken in the plane on which the camera is focused and if the lens-subject distance is at least 10 times the maximum breadth of the subject, the maximum, photographic error will be less than 1%. (For a theoretical discussion of this point see Appendix.) Under consideration of measurements later, it will be shown that at distances of 20 feet or more, measurements on the photograph are within the error of direct measurements.

Two points should be kept in mind: first, these distortions — apparent decrease in diameter and apparent increase in size of parts closer to the lens — are dependent solely on the size of the subject and the lens-subject distance. The focal length of the lens is immaterial. (It is assumed that the lens is as free of aberrations as possible.) For a given subject at a constant distance exactly the same distortion will be obtained from a 12-inch lens as from a 50-mm lens. The only difference will be the image size on the film. Second, both distortions decrease rapidly as the lens-subject distance is increased. From a practical viewpoint, it is more important to increase the lens-subject distance from 10 to 20 feet than to increase it from 20 to 30 feet. The error in measuring the diameter of a 50-cm sphere and the length of a line 25 cm closer to the camera than the plane of focus are given in tables 1 and 2. At infinity, there is no distortion, but for practical photographic anthropometry of a human subject, distortion in

TABLE 1

Ratio of photographic to actual diameter of a sphere 50 cm in diameter at 4 lens-subject distances

LENS-SUBJECT DISTANCE	RATIO
10 feet	.996
20 feet	1.000
30 feet	1.000
40 feet	1.000

the plane of focus is eliminated at 20 feet. Distortion of parts of a person closer to the camera than the plane of focus although almost eliminated at 40 feet are still appreciable.

The choice of a particular lèns-subject distance is a practical matter; there is no absolutely correct distance. However, when the subjects are adults, 20 feet should be considered a minimum. The 10 meters recommended by Tanner and Weiner ('49) allows greater freedom in taking measurements away from the plane of focus. Since space accommodations for such long distances is a major, practical problem and since such distances introduce difficulties in lighting, we have used a distance closer to the minimum, 21 feet.

TABLE 2

Per cent increase in a line 25 cm closer to the lens than the lens-subject distance

LENS-SUBJECT DISTANCE	PER CENT INCREASE	DIFFERENCE IN PER CENT
10 feet	9.36	4.80
20 feet	4.56	1.60
30 feet	2.96	.80
40 feet	2.16	

The reduction ratio

In order to convert easily the photographic to actual measurements, a simple reduction ratio is needed. If the image/subject ratio is 1/10, 1/20 or 1/50, it will be easier to obtain the actual measurements than if the ratio is 1/8.33. There is no right or wrong reduction ratio; the choice is a matter of convenience and circumstance. Smaller negatives are cheaper, but they require greater reduction which necessitates greater enlargement with greater loss of detail. Since we wanted detail, especially in the face, a moderately large negative was needed. We felt that a field 42 × 84 inches would be adequate for adults. At a 20:1 reduction this field occupies 2.1 × 4.2 inches on the film. With this arrangement one may use either $3\frac{1}{2} \times 4\frac{1}{4}$-inch film or, with a split back, 5 × 7-inch film. When this negative is enlarged two diameters the print has a 10:1 reduction and conversion of photographic to actual measurement is merely a matter of moving the decimal point.

Focal length of lens

The lens-subject distance and the reduction ratio determine the focal length of the lens as shown in the following formula

$$F = \frac{I\,D}{O + I}$$

where F = the focal length, I = image size, O = subject size, D = lens-subject distance. In the present instance the values are:

$$F = \frac{(1) \times (252 \text{ inches})}{20 + 1} = 12 \text{ inches.}$$

The only advantage of a long over a short focal length lens is that it produces a larger image at the same distance. With larger images measurements are easier to take; there is more detail, and greater enlargement is practical.

Lighting the subject

The lighting of the subject is always important, particularly if observations of muscle relief, quantity and distribution of hair, etc., are to be made. Changing the position of the lights can emphasize or eliminate critical details. It is a great mistake to think that any photograph is a reliable and comparable record of morphology. In order to take measurements accurately, the outline of the body must be clearly recorded and set off from the background. All parts of the body must be adequately illuminated, and the arrangement of the lights must be such that their position can be easily and consistently reproduced. In our opinion the arrangement which best fulfills these requirements was suggested by Mr. Nikolas Mauray. He recommends a single light directly above the lens, preferably mounted directly on the camera. This arrangement is easily reproduced as the position of the camera determines that of the light. Such flat, front lighting evenly illuminates the body and produces a thin shadow along the body outline. A white background brings this shadow into relief thereby clearly defining the body contour. In addition such lighting is ''soft'' enough that it does not ''burn out'' details of hair pattern, etc.

If the background is close to the subject, a shadow will be cast on it. This may be eliminated by separate lighting, by a beaded glass screen as suggested by Dupertuis, or by increasing the distance between subject and background.

In our opinion the most satsifactory light source is a high speed strobe unit. The exposure is so rapid that, even if the subject moves, each picture is clear and sharp. Also, there is neither heat, as from conventional photoflood, nor the objectionable blinding flash given by ordinary flash bulbs.

Selection of film

The choice of film is a matter of some concern because of the multiplicity of types differing in speed, grain, color sensitivity, and available size.[3] Many films meet the general requirements of being moderately fast and fine grained. We have obtained our best results with a pan film; although in some special study where a red-sensitive film would be unsatisfactory, ortho films could be used. In any event, the darkroom procedure, type of developer, time and temperature of development, should be carefully standardized for the film used. The procedures of the best portrait photographer are not necessarily the best for photographic anthropometry.

Use of standard color scales

In order to get comparable prints from different negatives it is necessary to include two scales in the picture. The first, the gray scale, consists of a series of 10 gradations from white to black. When the negative has been properly exposed, developed, and printed all 10 gradations are distinguishable. Over-exposure on either the film or the print will make at least two of the gradations blend. Comparability is determined from the gray scale, not from the shade of the subject.

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[3] A complete summary of all films, their characteristics, available size, recommended development, etc., may be found in ''Photo-Lab-Index,'' by H. M. Lester and J. S. Carroll; Morgan and Lester Co., N. Y. C.

The second scale, the color scale, consists of 5 rectangles, white, black, blue, yellow and red. In black and white photography, this scale is a permanent record of the type of film used, pan or ortho, while in color photography, it indicates the way in which the original colors were reproduced. In either case this scale gives a further control on the processing.

For photographic anthropometry these scales are essential. Without them *no precise comparability is possible.* Even with the most careful control of the lighting, film, and development there will be differences in photographs. Whether these are in the subject or in the photographic procedure can be determined only if the picture contains objects of known shade and color.

Selection of the printing paper

Variation produced by different types of paper processed in a variety of ways has been discussed by Tanner and Weiner ('49). Our experience was the same as theirs, namely, that ordinary paper shrinks and expands in an unpredictable manner. Such erratic variations can be eliminated by using a non-shrinkable base paper. This paper was developed for reproducing aerial maps where the precise rendition of undistorted details is essential. We have used Eastman's "Resisto" for contact prints and "Resisto Rapid" for enlargements although there are other brands available.

So far we have been concerned with the production of negatives and prints which can be used in the collection of anthropometric data. It was pointed out that although the exact equipment and its arrangement will depend upon the problem and the physical surroundings still there are certain essential principles which must be borne in mind. These essentials are: long lens-subject distance, even lighting from a constant position, gray and color scales, fine grain films, uniform processing, and non-shrinkable base paper.

The measurements

In order to simplify measuring and in order to identify any error which may be introduced, a size scale should be included in each photograph. The use of this scale is shown in plate 1, the measurement of stature. This measure may be taken from the photograph as accurately as with an anthropometer. There are several further advantages in the photographic method. The way the subject is posed can be seen. Krogman ('50) has called attention to the variation in stature due to posing. Variation of this sort is recorded in the photograph. Obviously, recording errors are eliminated and much information in addition to stature is given. There is no photographic error in this kind of measurement. The measure is read from a scale, and the stature would read the same, even if the picture were badly distorted. If a photograph merely records the length of a measure as taken by an instrument in the picture, *there is no photographic error at all.* Naturally, errors in posing and the use of instruments remain, but the photograph adds no new sources of variability and makes a permanent record, containing a great deal of information about the way the measurement was taken. Measurements of this sort, such as stature and sitting height, are not really photographic measurements at all, but photographic records of measures taken by instruments included in the picture. The more of the record that can be included in this way (name, address, date of birth, number, weight) the better. The number of measurements which may be taken in this manner is limited only by the investigator's ingenuity.

A second class of measurements are those which require the location of landmarks in the photograph. Error may come from the location of the landmark, the measurement, or from distortion in the picture. The comparison of some photographic measurements to traditional ones taken on the same people will make the problems clear (table 3).

Head length is clearly seen in either of the side views, standing or sitting. Measurement from brow to the upright presents no problem. But glabella does not project as far forward as the contour seen in the photograph. Therefore, the photographic lengths are longer than the ones taken with a caliper. However, the difference is not great, its cause can be seen in the photograph, and the photographic measurement should always be slightly larger than the caliper one. If the relation is reversed or if the caliper measurement is too much smaller, there has been a mistake in measuring or recording. It is in this way that the photographic measurements can be used to check many of the traditional ones, although the two measures are not precisely the same.

TABLE 3

Comparison of photographic and traditional measurements taken on two subjects (in cm)

MEASUREMENT	SUBJECT 1		SUBJECT 2	
	Photographic	Traditional	Photographic	Traditional
Stature	162	162.3	172	172.1
Sitting ht.	89	87.6	88	87.7
Chest br.	29	27.6	28	26.2
Head length	20	19.9	21	20.5
Head br.	18	15.6	16	14.8
Head ht.	13	12.6	12	11.8
Min. frontal	12	11.7	11	9.8
Bigonial diameter	13	10.5	12	10.2
Nose br.	4	3.3	4	3.6
Cervicale ht.	136	135.8	148	146.9
Trunk ht.	53	52.3	51	50.3
Upper arm	31	30.9	34	34.3
Forearm	23	24.0	26	25.4
Leg length	36	36.3	40	40.4

Head height offers an interesting comparison. This measurement has been one of the most difficult to take by traditional methods. In the photograph the anatomy of the ear is clearly seen but vertex is hidden in the hair. A line drawn parallel to the bottom of the stature indicator gives the upper landmark. The photographic measurement is larger than the traditional, probably due largely to the relatively wide wooden stature indicator compressing the hair much less than the thin metal end of the anthropometer. With a better stature indicator, head height can probably be taken more easily and accurately from the photograph than from the living. Even the present preliminary attempt shows that the photographic measurement can be made accurate enough to check measurements taken on the living. It should be remembered that the pose of the head may be seen in the photograph and this cause of error and uncertainty allowed for.

The third is a numerous class of measurements of which only a sampling is given so that the general difficulties and principles may be clear. Here the landmark cannot be located in the photograph unless it has previously been marked. Some such measurements are trunk height, length of the segments of the extremities, cervicale height, etc. In order to locate the landmarks we used skin pencils, water color paint, and finally gummed reinforcements shown in the illustrations. These gummed stickers, used to reinforce holes in notebook paper, are superior to the other indicators. The center of the circles defines a point; the taking of a measurement between two such points is easy and can be exactly duplicated. When crayon or paint is used the line must be wide in order to show clearly. The result is an ill defined area from which precise measurements cannot be taken. In addition the stickers are easier to apply and remove; they work equally well directly on the skin or on the clothing. Once removed, they leave no mark.

The application of the stickers requires no more training than the location of landmarks in traditional anthropometry. They possess the distinct advantage in that only one landmark has to be located at a time instead of two as in the ordinary technique. One finger locates the landmark and the sticker is applied, a far easier procedure than holding an instrument on two landmarks. Measures such as forearm length can be done more easily and accurately with stickers and photography than by traditional anthropometry.

Since any kind of marking on the skin moves relative to the underlying bones, the markers must be applied when the subject is in position to be photographed. However, with this caution in mind, different positions afford valuable checks on the accuracy of measurements. If femur length is marked in the extended position, it may be checked in a sitting pose. Gross error in placing the markers can be detected. Under optimum conditions the subject would be photographed standing, sitting, and with arms extended and bent. These different positions not only can be used to check measurements but permit a series of measurements useful in applied anthropology.

The measurements which belong to each of these three classes (where the photograph is a record of the instrument taking the measurement, where the photograph serves as a check on traditional technique, and where the measurement is taken later from the photograph) will depend on the posing and other technique used by the investigator. We have made no attempt to obtain all anthropometric data from our photographs. With use of auxiliary equipment the investigator can extend the scope of the photographic method as his problem dictates.

Posing

Posing for measurement presents no special photographic problems, and this paper makes no attempt to offer any particular system of posing. The general requirements are that parts being measured should be clearly visible and as near the plane of focus as possible. These specifications do alter the emphasis on particular poses. For example, the back view is much more useful for measurement than for the usual sort of observation. In the back view, if markers are used, inion, cervicale, iliac crest, posterior superior spine of ilium, 5th lumbar vertebra, sacrum, acromion process, spine of scapula, last rib, and segments of extremities are all available as landmarks. All these lie close to the same plane, and the back view is certainly one of the most useful for photographic measurement.

The requirement that points of interest be clearly visible leads to varying the pose as particular problems dictate. For example, if estimates of muscularity are desired, one or two poses in which the muscles are tensed are necessary. The comparison of muscle contours as seen relaxed and as they appear when in action gives a far better basis for estimating muscularity than either one alone. With special positions and lighting a great many individual muscles may be visualized. In Lockhart's "Living Anatomy" ('48) the problem of special positions for bringing particular muscles or groups of muscles clearly into view is systematically treated.

A standardized, high quality photograph is the closest approximation to the actual person which can be kept for future reference. If the photograph is properly taken, it will provide a more complete record than that given by measurements and observations alone and will serve as a check on many of the traditional measurements and observations. This check is possible because, as previously discussed and as can be seen in table 3, the measurements on the photograph are either the same as those taken on the person or bear a characteristic relation to the actual measurement (usually somewhat longer). If a scale photograph is available, impossibly large or small measurements can easily be detected and eliminated. Personal bias, as amount of pressure, location of nasion, will make one observer's measurements consistently larger or smaller relative to the photographic ones.

The difficulties in making consistent observations are so great and the advantages of having an accurate record of ear, nose, lips, many muscles, etc., so obvious, that the use of photographs to control traditional observations needs no comment. Frequently, some snapshots have been taken to supplement the recorded data. Our only point is that the utility of these can be increased many times if a scale photograph is substituted for the snapshot.

As stated previously our interest in photographic anthropometry stems directly from Sheldon's pioneering efforts. Time has shown certain weaknesses in his original photographic system: there was no scale whereby the absolute size of the subject could be determined; there were no means of ascertaining the way in which skin colors had been reproduced; there were no directions for consistent, standardized lighting, and there were no provisions for the control of the developing and printing process. Numerous statements in the primary descriptions of the somatotypes cannot be checked from the data given. For example, the limbs of endomorphs are said to be short. A check on the published photographs failed to confirm this, but the lack of landmarks made the error of measurement far too great for confidence in the result. As pointed out by Tanner and Weiner ('49) the lens-subject distance was too short, and the photographs undoubtedly contain many distortions not immediately apparent. Although Sheldon's system offers no adequate guarantee of photographic uniformity, it can easily be improved. For the purposes of constitutional investigation, it is far more important to adopt methods of photography which will do what is needed than to keep the present system for the sake of conformity. Surely the reduction of distortion, inclusion of scales, and the standardization of film, paper, and development are a minimum list of needed changes. Additional poses will contribute much information, especially on muscularity. The student of constitution needs more uniform photographs than does the general anthropologist.

DISCUSSION

Difficulties in comparisons arising from personal bias and error in measurements and observations can be reduced to a minimum by the use of scale photographs. In considering the value of photography for this purpose, two points should be borne in mind. First, certain measurements such as head breadth, bigonial diameter, and minimum frontal diameter cannot be taken on a photograph. The photograph only shows the surface, and structures beneath the surface may not affect the contours. The photograph, although a permanent record, is not a complete one. Therefore, photography rather than supplanting traditional anthropometry serves to augment it. Second, this investigation has been of a preliminary nature only. We were interested in the general conditions necessary to develop photography as a control for anthropological measurement and observation. Conventional and photographic measurements were taken and compared, but no attempt was then made to adjust either set of measurements to get greater agreement. Differences between the two methods can surely be reduced with practice.

However, it should be stressed that, no matter how standard and excellent the photograph, there are many problems which photography cannot solve. Reynolds and Asakawa ('50) have shown by x-ray that extreme endomorphs are very muscular; extreme mesomorphs and endomorphs in the series were equally muscular, but the endomorphs were so fat that muscle contours did not show in the photographs. Many of the problems raised by Sheldon's descriptions of somatotypes cannot be solved without better photographs. Many other preblems cannot be solved by photographs of the surface no matter how excellent.

We believe that the quality of the anthropological record can be greatly improved by the addition of highly uniform photographs of a known and simple scale. Such photographs will be useful in standardizing measurements and observations, in detecting bias, and increasing the amount of information available to the student of human constitution.

TECHNIQUE SUMMARY

By way of summary, the technique used in taking our illustrations (plates 1 and 2) will be given. This technique is given as a record of what was done. It is a preliminary, trial technique and undoubtedly many improvements can be made.

The camera was a Keith copying camera, 5 × 7, equipped with a sliding back so that two exposures could be made on each film. This camera lacked "swings" on the lens board and film holder so that the front and back of the camera were always parallel. It was mounted on an elevator type tripod equipped with a pan head; the camera could be raised or lowered without moving the tripod feet. Two spirit levels were used to make sure the head of the tripod was horizontal. Two perpendicular pencil lines intersecting at the center of the field were marked on the ground glass. These were used to align the camera.

A 12-inch, Eastman Commercial Ektar lens was used. To it was synchronized a single, portable strobe unit which was mounted directly above the lens. This unit supplied all the light on the front of the subject.

The posing stand was a light, wooden platform painted white. The vertical upright was a 2 × 4-inch on which was pasted a paper scale graduated in centimeters from zero to 200. The gray and color scales were attached to the upright. A small stool with adjustable foot rest was used for seated poses. On the front of the platform we attached cards showing the subject's weight in pounds (left) his number (right).

On the top of the platform three black lines were painted. The first extended across the platform from the base of the upright; the other two were perpendicular to the first. The subject was told to stand on the point of intersection at which the camera had previously been aligned. This was done by having the vertical line on the ground glass cover the line on the platform; the horizontal line on the ground glass was placed at the 100 cm mark on the upright. Probably the height of the camera should be adjusted for height of subject. The distance from the lens to the upright was 21 feet.

The background consisted of a heavy piece of white cloth suspended from the ceiling, three feet behind the platform. The bottom end of the background was attached to the back of the platform. Thus the background hung in a curve. It was separately lighted by two No. 2 photofloods which were shielded from the subject.

The film used was Eastman's Super Panchro-Press Type B. This is a high speed, fine grained pan film which is readily available as 5 × 7-inch cut film. The manufacturer recommends developer DK50 or DK60a, but as our light proved not to be as powerful as needed, special developing was used. This was only necessary because we did not realize at the time the light was purchased how long the lens-subject distance would have to be. The negatives were taken at f16, 1/50 second. This stop gives sufficient depth of field to cover the posing stand from front to back.

The negatives were enlarged two diameters on "Resisto Rapid," non-shrinkable base paper. Such prints have a reduction ratio of 10:1 and 1 mm on the print equals 1 cm on the subject.

As we were primarily interested in the technique of taking anthropometric pictures, we have not concerned ourselves with perfecting the posing stand or the poses. We still have considerable lack of consistency in the way the subject stands or sits. Undoubtedly many improvements could be made in the posing platform. However, as these two features, posing equipment and poses themselves, are determined by the problem under investigation, we have refrained from standardizing them.[4]

APPENDIX

Relation of lens-subject distance to maximum breadth of subject

As indicated in the text one type of distortion is this: at any finite distance the photographic diameter of a sphere will be less than the actual one. The amount of distortion depends on the ratio of the diameter of the subject to the lens-subject distance. The magnitude of such errors increases either as the lens-subject distance decreases (with size of the subject constant) or as the size of the subject increases (with lens-subject distance constant). The problem is to select a ratio (width of subject: lens-subject distance) so that the difference between the actual diameter and the photographic diameter will be less than the error of measurement.

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[4] Dupertuis and Tanner ('50) have developed a set of standardized poses which are applicable to some problems.

For present purposes it is assumed that stature is indicated by an instrument in the photograph and is not subject to photographic error. The maximum diameter of the subject will then be the bideltoid diameter. We assume that an acceptable measurement error should not be more than 1% and that the photographic error should be much less than that. Under these conditions the problem may be represented as in figure 2. Let O be the center of a circle with diameter XY (the bideltoid breadth); let L be the camera lens, and ML and NL be light rays tangent to the circle. The line VZ will be the apparent diameter of the circle on the negative. Let OL be the lens-subject distance. It is apparent that as OL increases or XY decreases, the points V and Z will approach Y and X respectively. A ratio XY: OL is to be selected so that VZ will be greater than 99% of XY.

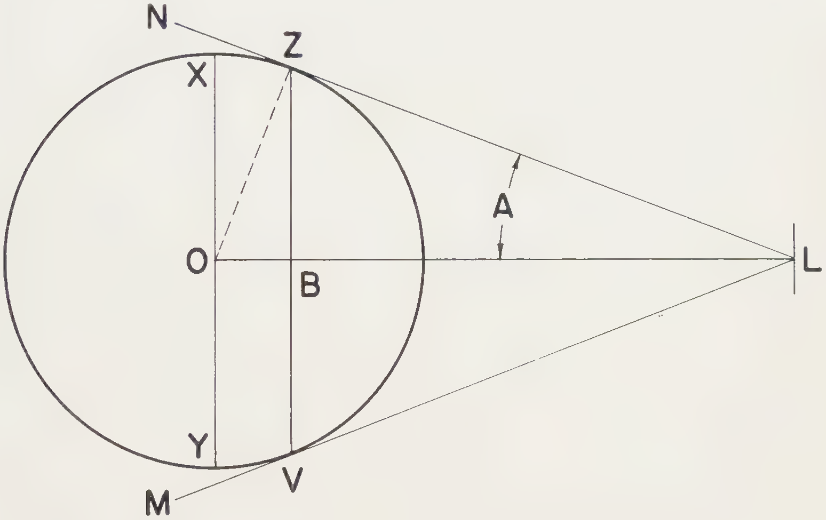

Fig. 2 Diagram for determining the ratio of maximum breadth of subject to lens-subject distance. See text for explanation.

Only the upper half of the figure will be needed for the analysis. Construct a line OZ, a radius of the circle equal to OX and perpendicular to NL. Let B be the point of intersection of OL and VZ, and let A equal one-half the angle made by light rays NL and ML. It follows that angle OZB must also equal A. Let $\frac{BZ}{OX} = \frac{999}{1000}$, our permitted photographic distortion error. Then

$$\frac{BZ}{OX} = \mathrm{Cos}\ \underline{|OZB} = \frac{999}{1000} = \mathrm{Cos}\ A$$

$$\mathrm{Cos}\ A = .999$$

$$A - 3^\circ$$

$$\text{but} \quad \mathrm{Sin}\ A = \frac{OZ}{OL}$$

$$\mathrm{Sin}\ 3^\circ = .05 = \frac{OZ}{OL}$$

$$\text{but} \quad OZ = OX$$

$$\therefore \quad \frac{OX}{OL} = .05 = \frac{5}{100} = \frac{1}{20}$$

$$\text{but} \quad OX = \frac{1}{2} XY$$

$$\therefore \quad \frac{\frac{XY}{2}}{OL} = \frac{1}{20}$$

$$\frac{XY}{OL} = \frac{1}{10}$$

That is, the ratio of the maximum breadth of the subject to the lens-subject distance should be as 1 : 10.

The average bideltoid breadth in adult, white males is about 50 cm according to Randall ('49). If subjects no broader than this are to be photographed the lens-subject distance would be 500 cm ($16\frac{2}{3}$ feet). A subject this broad at this distance, and with a 20 : 1 reduction ratio, requires a focal length lens of 9.5 inches. The next longer standard lens would be used.

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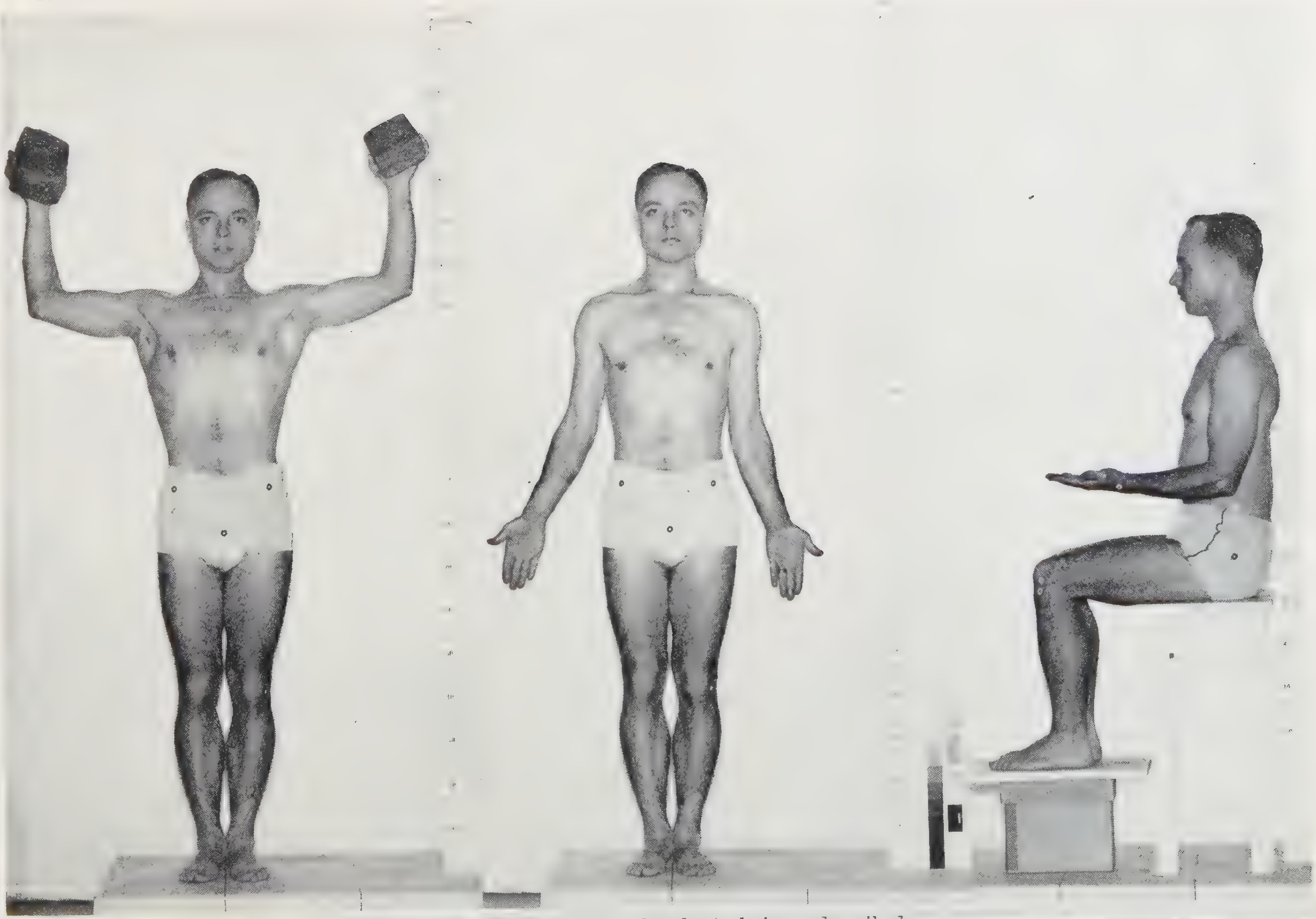
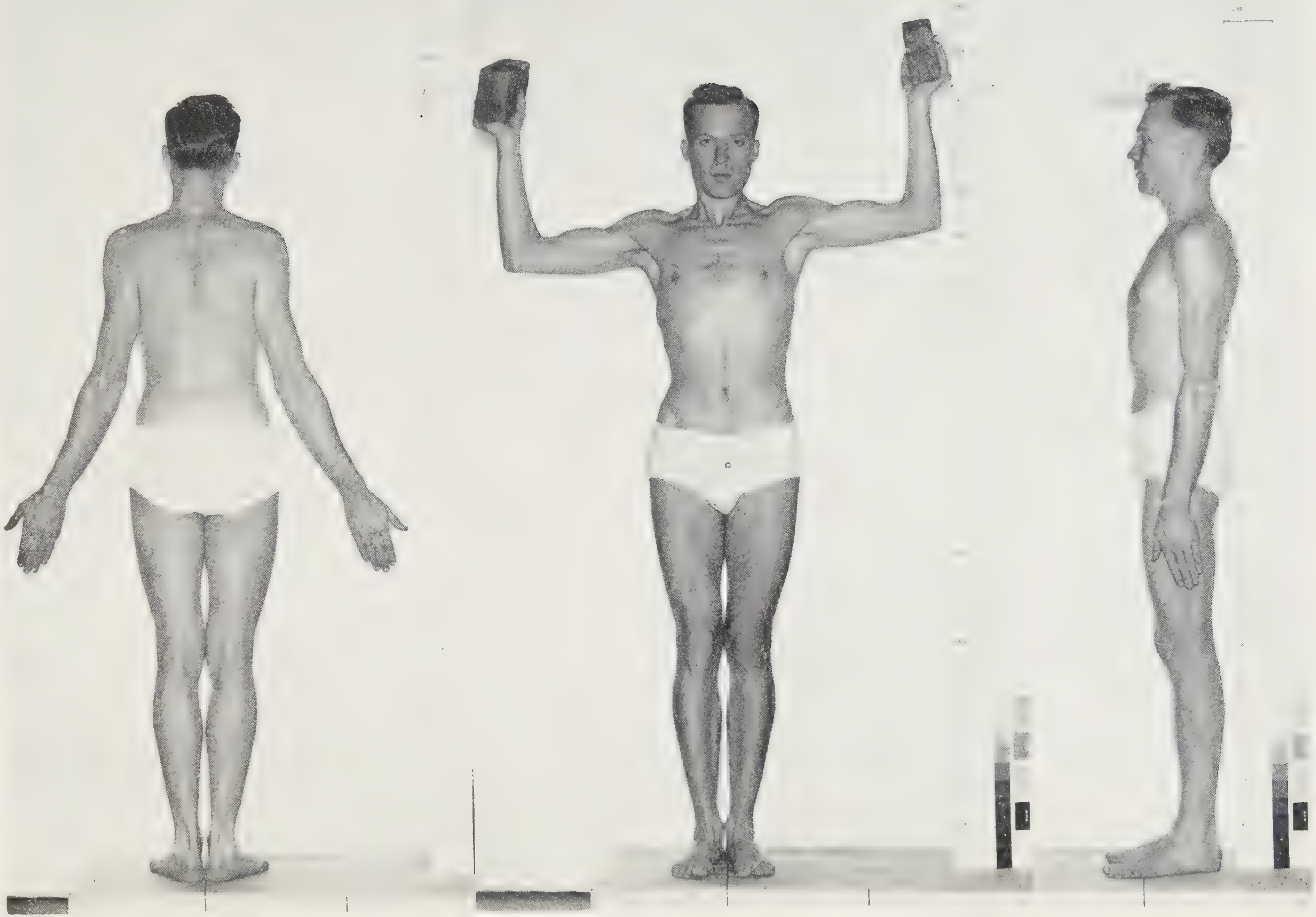
PHOTOGRAPHY: AN ANTHROPOMETRIC TOOL
J. A. GAVAN, S. L. WASHBURN AND P. H. LEWIS

PLATE 1



Specimen photographs taken by techniques described



Specimen photographs taken by techniques described.

* * *

Skeletons of Philippine Pygmies.— The Negrito skeletons which are the object of this study are preserved in the Musée de l'Homme, which received them from the Galerie d'Anthropologie of the Muséum National d'Histoire Naturelle, where they were acquired for the most part at the end of the last century, following the perilous explorations of the French scientists Marche (1883), Montano (1885), and still earlier (1855) La Gironière.

Some of their osteological parts have been studied as anatomical units: the pelvis by Verneau, the scapula by H. V. Vallois, the hand by Sarasin, the foot by Volkov, a few skulls by Quatrefages and Hamy. But no study of the collection as a whole has been attempted. I have undertaken this general study at the suggestion and under the supervision of Professor Vallois.

The Negrito skeletal remains studied represent 36 purebloods (15 male, 14 female, 7 juvenile) and 2 mixed bloods (male). Some of the skeletons are incomplete. Nevertheless, the collection permitted an interesting study, since such a large number of skeletal parts from this population have never before been assembled and examined.

The skeletons came from the island of Luzon, especially from the provinces of Bataán, Camarines Sur, Pampanga and the Tanauan mountains.—Genet-Varcin. Les Négritos de Luçon (Philippines). Étude ostéométrique; comparison entre les différentes races pygmées. L'Anthropologie, vol. 53, no. 1–2, 1949, pp. 33–67. [The two tables of measurements appended give only averages for each sex and fail to indicate the numbers of individuals involved in each calculation. — Ed.]

BRIEF COMMUNICATIONS

ON VARIOUS LEVELS OF OBJECTIVITY IN GENETICAL ANTHROPOLOGY[1]

JOSEPH B. BIRDSELL
University of California at Los Angeles

In the December issue of this journal, T. D. Stewart ('51), under the title "Objectivity in Race Classifications," has with some pertinence questioned the degree of objectivity now characterizing the genetical approach to racial classification and to human evolution. His argument rests upon the contention that racial classifications, expressed in frequencies of genetic variants for populations, parallel those based upon phenotypic data *because* the populations genetically sampled have been chosen for their phenotypic characteristics. Thus he concludes:

> I repeat, then, that blood group data have been subjected to selection largely, if not entirely, on the basis of phenotypes.

The purpose of this communication is to agree broadly with Stewart's theme that a genetical approach to human racial taxonomy contains non-objective aspects, but further to disagree in amiable terms as to the nature of the subjectivities involved. Proponents of the genetical approach, and I place myself among these, have tended to oversimplify population genetics by minimizing its essential complexities, and by remaining inexplicit about the detailed stages needed for a genetical analysis of the problems of race formation in man. Enthusiastic salesmanship for a new analytical approach may be necessary initially, but sober evaluation must be attempted in time to provide realistic perspectives.

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[1] Since submitting this communication, the writer has become aware that S. L. Washburn has expressed some similar ideas in a paper given at the Wenner-Gren Symposium in June, and would like to draw the reader's attention to that paper.

One may disagree at several points with Stewart's belief that major genetical categories of race, as presented by Boyd ('51), correspond with the classical categories of race *because* the bases of population selection are phenotypic in character. Having been concerned with Simmons and his co-workers in the formulation of a project involving collaborative genetical surveying in the Pacific area (Birdsell, '51), I can state that populations may be chosen for sampling for a wide variety of reasons. Some of the primary variables involved in sampling choices include:

1. *Phenotypic characteristics* of the population as a clue to affinities with previously recognized racial groups;
2. *Cultural considerations* which may identify marginal, transitional, or nuclear groups of populations in terms of material culture, social factors or linguistics;
3. *Geographical influences* which assist in defining spatial relationships of populations;
4. *Other environmental variables* which may provide a significant test for various aspects of adaptedness in human populations; and
5. *Isogenic patterns* which in themselves may be suggestive of new critical areas for sampling.

It will be noted that the factor of phenotypic characteristics is but one of a number of variables in sampling choice. Nor is it totally essential to the genetical approach. In theory it would be feasible to eliminate the phenotypic factor entirely. As an extreme position, it would be possible to work exclusively within the field of any single one of the above categories. One could use language. Or an environmental factor: temperature stressing. Or geographical barriers. These, and other alternatives, represent possible sampling schemata, but each would be inefficient, resting upon too narrow a base. It is good and ultimately economical practice to utilize all types of variables, including the phenotypic, in choosing populations for sampling. But the phenotypic category is in itself neither essential nor necessarily causally related to the genetical results obtained.

For some genetical purposes phenotypic variables may be ignored. In my projected return to Australia for two years' work among the aborigines, the primary variable is a spatial factor; the materials for the investigation are the varying gene frequencies of half a hundred tribal isolates contiguous in space and traversing a wide range of environments. The research goal is the preliminary identification of the results of the evolutionary processes of selection and drift as they have affected these populations. As a historical consequence of the pattern of white colonization, these data must be sought for today in the northwestern portion of the continent. A similar number of tribes from any other transect could serve as well, even though differing in phenotypic traits.

There are further theoretical reasons why the results of genetical surveys need not coincide with phenotypically defined racial categories. Genes, at loci which are not linked, can vary independently in the changes in frequencies induced by evolutionary forces. Even at linked loci, unless the linkage is extremely close, and the time interval so short that crossing-over equilibrium has not been attained, alleles can vary as independently as those at unlinked loci. Thus there is no reason in theory why patterns in frequencies for serological genes should parallel either configurations in phenotypic traits, or the frequencies of the undefined genes responsible for their expression.

Finally, there are examples of populations which resemble each other phenotypically in a general way, but for whom the gene frequencies, at the loci tested so far, differ markedly. American Indians and Eastern Asiatics are such a contrasting pair. Polynesians and Micronesians form another. If it should be demonstrated finally on a statistically adequate basis that genetical definitions of racial populations in fact do parallel those now defined phenotypically, it will not demonstrate direct causal relationship between the two sets of factors. Rather it will be evidence for the effects of broad regional adaptedness in causing differentiation in both genetical and phenotypical configurations.

One may agree with the cogent statement of Strandskov and Washburn ('51) that human population genetics and modern human anatomy should supplement each other, and that both are necessary for a full understanding of race. It is becoming clear that genetically simple characters, such as are now the only materials available for use in human population genetics, and which are most useful for studying certain evolutionary processes, may on the other hand give biased and blurred impressions of population relationships distant in time or space. For the latter type of analysis, genetically complex traits may serve better, even though they offer little hope of detailed genetic definition. Thus morphological characteristics and, to a lesser degree, metrical measurements may continue to provide important information for studies both in racial taxonomy and the processes of race formation. They must be utilized within a modern evolutionary framework, however, and are in need of reanalyses designed to provide insight as to the effect of functional and other environmental influences upon their expression. This would not be a continuation of the classical approach to race, but rather a salvaging of some phenotypic characters formerly utilized. Thus, in Australasia and the island world of the mid-Pacific, helically curled hair (almost certainly a genetically complex trait), is a better marker for a Negritic component in populations than are serological genes tested to date.

To return to the genetical approach to racial analysis, Dobzhansky ('51) has succinctly framed the problem in the following terms:

> *Races may be defined as Mendelian populations of a species which differ in the frequencies of one or more genetic variants, gene alleles, or chromosomal structures.* Race differences of diverse orders of magnitude are observed. . . . Two logically and methodologically separated problems should be distinguished in racial studies. First, one may wish to decide whether certain populations of a species are racially distinct. This can be answered, and conclusively, by a statistical comparison of the incidence of genetic variants in the gene pools of the population in question. . . . Second, one may ask into how many races a species is divided. This is a matter of convention and convenience.

In terms of this definition, it is now necessary to define human races primarily in terms of differing frequencies of alleles. First in this two-fold approach, it is but necessary to amass data upon the genetic differences displayed by the various critical human populations of the world. But note that even at this modest level of research, the data may be affected by subjectivities for a series of reasons:

1. The definition of the *limiting boundaries of the population* tested are not genetically objective. Even in those rare cases where insular groups of limited size provide the test subjects, rates of inter-population gene flow have seldom been calculated. Since the definition of the unit of population varies with the rate of gene exchange between groups, it is clear that complete objectivity has not been obtained.
2. *Techniques must yield results* with a high degree of accuracy. Even with techniques now used it is doubtful if complete and absolute objectivity is always obtained.
3. The *mathematical techniques* utilized for the estimation of gene frequencies from the frequencies of phenotypes contain possible bias as applied to specific populations. These equations operate well only within the limits of the basic assumptions and the probability statistics upon which they are dependent for their mathematical simplicity. Thus the formula for calculating the gene frequencies at the O-A-B locus may give only approximate values for the true gene frequencies of a specific population, and the maximum likelihood method for calculating the Rh gene frequencies is an even less direct method of estimation. Serological advances which allow the direct identification of all genotypes in allelic systems will in time provide the solution for these difficulties.
4. At the present time, the 10 loci available for determining population gene frequencies in man *are not statistically adequate.* They do not represent an adequate sample numerically of the total variable loci significant for the analysis of racial differences. Nor is there any assurance that they are representative of those which may ultimately prove most informative concerning the processes of race formation. The available

loci have been stumbled upon by serologists, and there is no certainty that they are essentially random in terms of a specific problem. Thus a non-objective bias, in terms of the limited number of loci and the preponderance of genetically simple characters, may prove to have existed at this stage of development in human population genetics.

The admission of non-objective factors in current genetical anthropology does not imply that classical anthropology was lacking in yet more subjective elements. The subjectivities here conceded occur at a higher level than those inherent in the older method. But it is fair to grant that genetical anthropology will be more nearly objective when population limits are genetically identified more accurately; when techniques are improved both in objectivity and power of genetical discrimination; and when, most importantly, the loci available for testing are greatly multiplied in number and so distributed as to include both genetically simple and complex traits, including those affecting the expression of structural, morphological and physiological variation. Human population genetics today can define genetical differences between human populations with reasonable objectivity, but a mere beginning has been made and much more work will be required to raise its general level of objectivity to a proper height. To this degree one can agree with Stewart's contention.

The full implications of Dobzhansky's second point do not lie on the surface. When the data become available, the human populations of the world may indeed be divided into races according to convenience and convention. The categories created will vary with the primary purpose motivating the taxonomist. One may agree with Brew ('46) that, within limits, the more taxonomies the better. Such flexibility in the creation of taxonomies, of course, implies a form of subjectivity, a subjectivity which also occurs in the taxonomic aspects of other sciences. Non-objectivity of this kind may tend to be minimized by the applications of new methodologies, such as Womble's differential systematics ('51), but one may doubt that it will be totally eliminated.

The non-explicit and most important portion of Dobzhansky's statement concerns the means utilized to evaluate the significance of deviations in gene frequencies between human populations. Differences may be recorded ultimately with high objectivity, but any interpretation of the meaning of such differences will remain dependent upon detailed knowledge of the effects of the evolutionary processes which produce them. Today, for genes at loci useful in population genetics, we have determined in no single instance a rate of mutation, selection, or drift in natural human populations. Actual rates for all three processes must be obtained for different populations under varied conditions, and for various genes at numerous loci, if interpretative procedures are to be soundly grounded. This is the most pressing problem for the current generation of human population geneticists. The creation of detailed taxonomic constructs can well be delayed until the controlling data for their proper framing (that is, an evaluation of the ways in which evolutionary forces operate under various natural circumstances) can be approximated. It is paradoxical but true that to create a genetical taxonomy for man one must first solve the much more important questions as to how the evolutionary processes operate in detail. Progress will come through research designed to provide first approximate answers and later more refined measures. Advances will come through bracketing procedures designed progressively to narrow the range of error in estimation.

In the above discussion, the evolutionary force of hybridization has been intentionally omitted. In the first place, the theoretical basis for its interpretation is reasonably well developed. Secondly, it is the single force to date which has been primarily utilized in the interpretation of both metrical and genetical data in man. It is not too much to say that the apparent discrepancy between the taxonomy of the older anthropology and the genetical approach may result largely from the sole utilization of hybridization to explain all observed population differences. Sufficient genetical data have already been gathered to indicate that hybridization alone is inadequate to explain known differences between peoples.

It becomes evident that genetical anthropology is not a simple approach to the evolution of man. Solace may be found in the fact that evolution is not a simple process. The admission of various levels of subjectivity in human population genetics is not damning; subjectivity in comparable forms occurs at all levels of science. As compared to classical physical anthropology, with its emphasis on the individual rather than the population, its preoccupation with typologies, its use of unanalyzed phenotypic traits, and its very paucity of terms for the identification of the basic forces of evolution, human population genetics is a progressive step forward. If its techniques are limited in number and reveal a certain immaturity in development, it has the virtue of asking important questions, and the hope of providing some of the proper answers. Despite Stewart's closing line ('51) one may properly maintain that the change now in progress from the older classical anthropology to human population genetics does in fact loom larger than gradual evolution; it stands at least as quantum evolution, and perhaps as a revolution.

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REVISED VERSION OF UNESCO STATEMENT ON RACE

HARRY L. SHAPIRO
American Museum of Natural History, New York

Under the date of July 18, 1950, the United Nations Educational, Scientific and Cultural Organization issued a formal statement designed to represent an authoritative and scientific declaration on race. At a conference held in Unesco House, Paris, in December, 1949, the content of the document had been discussed and agreed upon by a panel consisting of the following distinguished scholars:

Professor Ernest Beaglehole, New Zealand
Professor Juan Comas, Mexico
Professor L. A. Costa Pinto, Brazil
Professor Franklin Frazier, United States
Professor Morris Ginsberg, United Kingdom
Dr. Humayun Kabir, India
Professor Claude Levi-Strauss, France
Professor Ashley Montagu, United States (Rapporteur)

Its final form was prepared by Professor Ashley Montagu, who had the benefit of the advice of

Professor Hadley Cantril
Professor E. G. Conklin
Professor Gunnar Dahlberg
Professor Theodosius Dobzhansky
Professor L. C. Dunn
Professor Donald Hager
Professor Julian S. Huxley
Professor Otto Klineberg
Professor Wilbert Moore
Professor H. J. Muller
Professor Gunnar Myrdal
Professor Joseph Needham
Professor Curt Stern

This statement on race was solicited by Unesco as part of its campaign to combat the evils of racism. This is a function which Unesco regards as one of its fundamental purposes. At its organization in 1945 it was specifically charged with this task, which was later reaffirmed by various resolutions adopted by the General Conference in 1948 and 1949. In the latter year the General Conference definitely committed Unesco "to study and collect scientific materials concerning questions of race"; "to give wide diffusion to the scientific material collected," and "to prepare an educational campaign based on this material."

Since the concepts and notions about race that are current in the world are extraordinarily diverse and frequently ill-founded, Unesco was confronted with the necessity of securing a precise statement on the subject by specialists which would (1) represent the best scientific opinion and (2) set up a corpus of general agreement.

The publication of the Statement, which it was hoped would achieve these ends, met however with considerable criticism from professional students, particularly in England and France.[1] There were at first no published comments on the part of American physical anthropologists. This apparent failure to react favorably or otherwise to the Statement may, I think, be attributed to its inadequate distribution among American specialists on race.

Since Unesco's purpose is to obtain, if possible, a declaration widely acceptable to recognized authorities and not to defend or support any particular document, Dr. Alfred Métraux, who is in charge of Unesco's activities in this matter, decided to call a second conference to reconsider the entire subject and attempt to frame another statement on race that might prove more acceptable without sacrificing the highest standards of scientific objectivity. Since one of the objections to the first statement was the relative paucity on the panel of specialists on the biological aspects of race, every effort was made to secure a representative roster. This, incidentally, did not prove as simple as might be supposed. The group which met for 5 days, June 4 to June 8, 1951, at Paris, included the following:

Physical anthropologists

Professor R. A. M. Bergman, Netherlands Anthropological Society, Amsterdam
Professor Ashley Montagu, Rutgers University, New Brunswick, New Jersey
Dr. Eugène Schreider, Laboratoire d'Anthropologie Physique, Institut de Paléontologie humaine, Paris
Dr. Harry L. Shapiro, Chairman, Department of Anthropology, American Museum of Natural History, New York
Dr. J. C. Trevor, University Lecturer in Anthropology, Faculty of Archaeology and Anthropology, Cambridge (U.K.)
Dr. Henri V. Vallois, Professeur au Museum d'Histoire naturelle, Directeur du Musée de l'Homme, Paris
Professor S. Zuckerman, Department of Anatomy, Medical School, Unisity of Birmingham

Geneticists

Professor Gunnar Dahlberg, Director, State Institute for Human Genetics and Race Biology, University of Uppsala, Sweden
Professor L. C. Dunn, Department of Zoology, Columbia University, New York
Professor J. B. S. Haldane, Department of Biometry, University College, London
Professor Hans Nachtsheim, Institut für Genetik, Freie Universität, Berlin

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[1] See, for example, *Man*, Vol. 50, October, 1950; Vol. 51, January, April, 1951.

Serological anthropologist

Dr. A. E. Mourant, Director, Blood Group Reference Laboratory, Lister Institute, London

Professor Henri Vallois was elected chairman and Professor Leslie C. Dunn rapporteur. The discussions were carried on in French and English, with the translations expertly managed by experienced Unesco linguists.

As a participant I can vouch for the painstaking and detailed effort that went into the work of the conference. The statement as it took shape does not, of course, represent fully the ideas nor the characteristic language of any one of us. In the very nature of the circumstances it could only form a kind of core of common agreement expressed in carefully considered terms. The fact that the individuals in a group as large as this, representing so many different points of view, did not in the end balance each other off, speaks strongly for the positive conclusions upon which they could agree. This must be kept in mind in reading the statement finally adopted. Any suggestions or emendations should be addressed to Dr. Alfred Métraux, Unesco House, 19, Avenue Kléber, Paris 16, France.

The statement adopted at Paris, June, 1951, was subsequently critically reviewed by Professor Julian Huxley and professor Theodosius Dobzhansky and revised by subsequent consultation with various members of the panel, and in the light of suggestions from other anthropologists and geneticists. All changes were approved by the members of the panel. Professor Leslie C. Dunn as Rapporteur had the difficult task of preparing the statement which is given below. This final version was approved by the panel as of May 26, 1952.

STATEMENT ON RACE

1951

1. Scientists are generally agreed that all men living today belong to a single species, *Homo sapiens,* and are derived from a common stock, even though there is some dispute as to when and how different human groups diverged from this common stock.

The concept of race is unanimously regarded by anthropologists as a classificatory device providing a zoological frame within which the various groups of mankind may be arranged and by means of which studies of evolutionary processes can be facilitated. In its anthropological sense, the word "race" should be reserved for groups of mankind possessing well-developed and primarily heritable physical differences from other groups. Many populations can be so classified but, because of the complexity of human history, there are also many populations which cannot easily be fitted into a racial classification.

2. Some of the physical differences between human groups are due to differences in hereditary constitution and some to differences in the environments in which they have been brought up. In most cases, both influences have been at work. The science of genetics suggests that the hereditary differences among populations of a single species are the results of the action of two sets of processes. On the one hand, the genetic composition of isolated populations is constantly but gradually being altered by natural selection and by occasional changes (mutations) in the material particles (genes) which control heredity. Populations are also affected by fortuitous changes in gene frequency and by marriage customs. On the other hand, crossing is constantly breaking down the differentiations so set up. The new mixed populations, in so far as they, in turn, become isolated, are subject to the same processes, and these may lead to further changes. Existing races are merely the result, considered at a particular moment in time, of the total effect of such processes on the human species. The hereditary characters to be used in the classification of human groups, the limits of their variation within these groups, and thus the extent of the classificatory sub-divisions adopted may legitimately differ according to the scientific purpose in view.

3. National, religious, geographical, linguistic and cultural groups do not necessarily coincide with racial groups; and the cultural traits of such groups have no demonstrated connection with racial traits. Americans are not a race, nor are Frenchmen, nor Germans; nor *ipso facto* is any other national group. Muslims and Jews are no more races than are Roman Catholics and Protestants; nor are people who live in Iceland or Britain or India, or who speak English or any other language, or who are culturally Turkish or Chinese and the like, thereby describable as races. The use of the term "race" in speaking of such groups may be a serious error, but it is one which is habitually committed.

4. Human races can be, and have been classified in different ways by different anthropologists. Most of them agree in classifying the greater part of existing mankind into at least three large units, which may be called major groups (in French *grand'races,* in German *Hauptrassen).* Such a classification does not depend on any single physical character, nor does, for example, skin color by itself necessarily distinguish one major group from another. Furthermore, so far as it has been possible to analyze them, the differences in physical structure which distinguish one major group from another give no support to popular notions of any general "superiority" or "inferiority" which are sometimes implied in referring to these groups.

Broadly speaking, individuals belonging to different major groups of mankind are distinguishable by virtue of their physical characters, but individual members, or small groups, belonging to different races within the same major group are usually not so distinguishable. Even the major groups grade into each other, and the physical traits by which they and the races within them are characterized overlap considerably. With respect to most, if not all, measurable characters, the differences among individuals belonging to the same race are greater than the differences that occur between the observed averages for two or more races within the same major group.

5. Most anthropologists do not include mental characteristics in their classification of human races. Studies within a single race have shown that both innate

capacity and environmental opportunity determine the results of tests of intelligence and temperatment, though their relative importance is disputed.

When intelligence tests, even non-verbal, are made on a group of non-literate people, their scores are usually lower than those of more civilized people. It has been recorded that different groups of the same race occupying similarly high levels of civilization may yield considerable differences in intelligence tests. When, however, the two groups have been brought up from childhood in similar environments, the differences are usually very slight. Moreover, there is good evidence that, given similar opportunities, the average performance (that is to say, the performance of the individual who is representative because he is surpassed by as many as he surpasses), and the variation round it, do not differ appreciably from one race to another.

Even those psychologists who claim to have found the greatest differences in intelligence between groups of different racial origin, and have contended that they are hereditary, always report that some members of the group of inferior performance surpass not merely the lowest ranking member of the superior group, but also the average of its members. In any case, it has never been possible to separate members of two groups on the basis of mental capacity, as they can often be separated on a basis of religion, skin color, hair form or language. It is possible, though not proved, that some types of innate capacity for intellectual and emotional responses are commoner in one human group than in another, but it is certain that, within a single group, innate capacities vary as much as, if not more than, they do between different groups.

The study of the heredity of psychological characteristics is beset with difficulties. We know that certain mental diseases and defects are transmitted from one generation to the next, but we are less familiar with the part played by heredity in the mental life of normal individuals. The normal individual, irrespective of race, is essentially educable. It follows that his intellectual and moral life is largely conditioned by his training and by his physical and social environment.

It often happens that a national group may appear to be characterized by particular psychological attributes. The superficial view would be that this is due to race. Scientifically, however, we realize that any common psychological attribute is more likely to be due to a common historical and social background, and that such attributes may obscure the fact that, within different populations consisting of many human types, one will find approximately the same range of temperament and intelligence.

6. The scientific material available to us at present does not justify the conclusion that inherited genetic differences are a major factor in producing the differences between the cultures and cultural achievements of different people or groups. It does indicate, on the contrary, that a major factor in explaining such differences is the cultural experience which each group has undergone.

7. There is no evidence for the existence of so-called "pure" races. Skeletal remains provide the basis of our limited knowledge about earlier races. In regard to race mixture, the evidence points to the fact that human hybridization has been going on for an indefinite but considerable time. Indeed, one of the processes of race formation and race extinction or absorption is by means of hybridization between races. As there is no reliable evidence that disadvantageous effects are

produced thereby, no biological justification exists for prohibiting intermarriage between persons of different races.

8. We now have to consider the bearing of these statements on the problem of human equality. We wish to emphasize that equality of opportunity and equality in law in no way depend, as ethical principles, upon the assertion that human beings are in fact equal in endowment.

9. We have thought it worth while to set out in a formal manner what is at present scientifically established concerning individual and group differences.

(a) In matters of race, the only characteristics which anthropologists have so far been able to use effectively as a basis for classification are physical (anatomical and physiological).

(b) Available scientific knowledge provides no basis for believing that the groups of mankind differ in their innate capacity for intellectual and emotional development.

(c) Some biological differences between human beings within a single race may be as great as or greater than the same biological differences between races.

(d) Vast social changes have occurred that have not been connected in any way with changes in racial type. Historical and sociological studies thus support the view that genetic differences are of little significance in determining the social and cultural differences between different groups of men.

(e) There is no evidence that race mixture produces disadvantageous results from a biological point of view. The social results of race mixture, whether for good or ill, can generally be traced to social factors.

NEANDERTHAL AND THE MODERN TYPE OF MAN

M. F. ASHLEY MONTAGU
Rutgers University

In his paper entitled "The Place of Neanderthal Man in Human Evolution" Howell ('51) rejects the idea "that Neanderthal forms were collaterals of modern man, believed to have become extinct" on the ground that "Current work in England with the fluorine method of dating has shown that several of the key specimens of presumed early modern man (Galley Hill, Baker's Hole, Piltdown, and possibly the London skull) are far more recent than was hitherto believed."

This argument is not valid, for it neglects recognition of the fact that the claim to antiquity of (the type resembling) modern man does not rest on such forms as Galley Hill, Baker's Hole, or Piltdown. Piltdown, incidentally, might be placed in the suspense account, for this type may yet turn out to belong to the second interglacial.

The claim to antiquity of the modern type of man now rests principally on fossils such as Fontéchevade I and II. Howell states that "the characters of Fontéchevade differ little from those of the early Neanderthals." I find this statement somewhat surprising in view of Vallois' ('49) clear account of Fontéchevade I and II in this Journal. Vallois left no doubt that the form and dimensions of Fontéchevade I and II were in no way Neanderthaloid. Of Fontéchevade II Vallois writes "reconstruction of the frontal . . . therefore confirms this important fact: he had a forehead constructed on the same type as our own, and totally devoid of the torus so characteristic of the Neanderthals" (p. 348). Of Fontéchevade I, Vallois writes "The essential fact is the absolute absence of a supraorbital torus: the glabella and brow ridge are less developed than in the Upper Paleolithic Europeans, or even the majority of Europeans of today" (p. 352).

In December, 1949, through the courtesy of Professor Henri Vallois I was able to examine the Fontéchevade remains, and I should here like to emphasize the fact that if there can be the slightest doubt about the status of Fontéchevade II, there can be none whatever concerning Fontéchevade I. Howell omits any reference to this important fragment. The latter consists of a beautifully preserved portion of the glabellar region of the frontal bone over the root of the nose together with a part of the left orbit including a supraorbital margin which is as sharp and as delicately made as in the most gracile of modern female crania. The bone is that of an adult or possibly a late adolescent. In morphology and thickness it is in all respects identical with that of modern caucasoid man.

The Fontéchevade horizon is third interglacial and the associated industry is Tayacian, i.e., *pre*-Mousterian. Stratigraphically and culturally Fontéchevade in France *precedes* the earliest Neanderthal types. The facts do not lend the least support to Howell's view that "the characters of Fontéchevade differ little from those of the early Nenaderthaloids." Fontéchevade I constitutes the complete rebuttal of that statement. As for Fontéchevade II that seems to me to do so, too, but concerning Fontéchevade I for those who have actually seen and studied the fragment there can be not the least doubt of its status — it is indistinguishable from the same part of the skull in a modern European female.

Unfortunately the age of the Steinheim skull is uncertain. It may be second interglacial or third interglacial. In any event, it is generally agreed that Steinheim contains an interesting medley of neanderthaloid and neanthropic traits. Howell thinks that there is a resemblance between Swanscombe and Steinheim; if there is a resemblance it is limited to the occiput; the form of the parietals differs and the Steinheim frontal simply will not — formwise — fit the Swanscombe parietal, whereas a frontal from contemporary man will (Le Gros Clark, '49). This does not deny the possibility that they may be related.

The evidence provided by the fluorine tests has certainly removed the claims of a number of specimens to be extremely ancient representatives of the modern type of man. But Fontéchevade man cannot be so lightly brushed aside, and I suggest that until evidence to the contrary be forthcoming Fontéchevade man must be regarded as at the very least a modern form which not only preceded but was collateral to Neanderthal man.

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CRANIAL DEFORMATION IN MINORCA, BALEARIC ISLANDS

L. CABOT BRIGGS AND DAMIAN SANCHO

ONE FIGURE

The practice of binding the heads of infants survives to this day in the Island of Minorca, where it is carried out in the following manner.[1] A tailored band of cloth, known as a *toocādo*[2] (see fig. 1) is placed around the head of the infant at birth and is continued in use for a period of from 6 to 8 weeks, more often the latter. This band envelopes the forehead from just above the brow ridges to a little above the hair line, while its lateral ribbon-like extensions are passed backward around the sides of the head and outside the tops of the ears, crossed over the point of greatest rearward extension of the occiput, brought forward again, and tied in a bow knot over the forehead at about the level of the hair line.

The band itself is made of very fine linen lightly starched. A V-shaped section is cut out of the middle of the upper half and the edges sewn together so as to give a snug fit over the forehead. A strip extending along the middle of the bottom edge in front, about two centimeters deep and 10 wide, is cut out and replaced by a piece of lace which gives slightly increased elasticity at this point. Linen and lace alike are white. When the *toocādo* is discarded for good it is washed, starched and frugally stored away for use on future offspring.

The *toocādo* is first put on, as soon as the child is born, by the midwife who continues to perform this function until the umbilical cord drops off, at which time the mother takes over. It is changed each time the child is washed, once a day. "It is pulled tight, but not tight enough to made the child cry." Experiments with informants who have used it on their own children indicate that the pull exerted on the ribbons in the process of application, and maintained by the knot, is of the order of one and one-half to two pounds at least.

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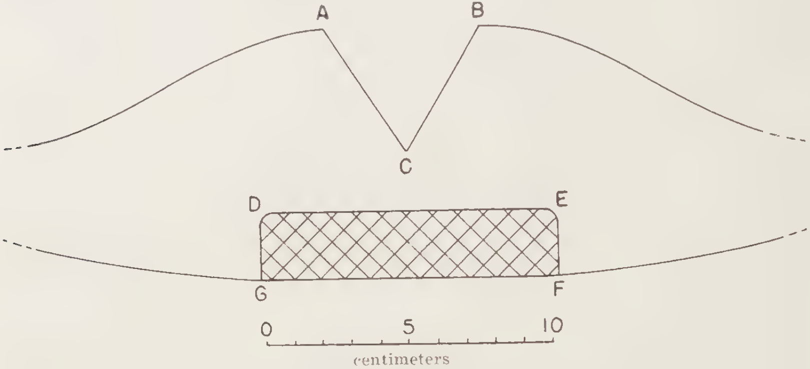
[1] The data given here were collected in the region of the town of Ciudadella, at the extreme western end of the Island.

[2] Noun, masculine; presumably from the local dialect verb *tōōca*, meaning essentially "to touch," but used here in its derived meaning of "retouch," that is to say "remodel." (Spellings are given in the nearest English phonetic equivalent.)

The purpose of the *toocādo* seems to be wholly aesthetic: it is used ostensibly "to hold the bones of the skull in place," but also to reduce both occipital protuberance and lambdoid flattening, to produce "a well rounded forehead," and "to keep the ears from sticking out."



The edges A-C and B-C are sewn together in the finished article.
The area D-E-F-G is cut out and replaced by a band of lace.
The ends taper off sharply to ribbons about 2 cm wide.

Fig. 1 Details of head-binding band used in Minorca.

The use of the *toocādo,* although still common, is dying out as one would expect. The wealthier land owning "older families" are the most reluctant to abandon it. Apparently there is no religious feeling for or against its use, and there seems to be no ceremony or ritual of any sort, however trivial, connected with its first application, later use, or final removal.

BLOOD GROUP FACTORS IN ANTHROPOID APES AND MONKEYS

I. STUDIES ON A CHIMPANZEE, "PAN"

ALEXANDER S. WIENER
Serological Laboratory of the Office of the Chief Medical Examiner of New York City, and the Department of Immunology of the Jewish Hospital of Brooklyn

Recently, a sample of blood from a chimpanzee named Pan was made available to us by Mr. James A. Gavan of the Yerkes

Laboratories of Primate Biology, Inc., of Orange Park, Florida. The opportunity was taken to restudy the blood group factors in the blood of this chimpanzee, using the more recently developed conglutination, anti-globulin and proteolytic enzyme techniques. The following is a brief statement of the significant findings.

The A-B-O groups. The blood of chimpanzee Pan belongs to group O. Anti-A and anti-B reagents prepared from rabbit immune sera do not clump his blood cells. Certain anti-A and anti-B sera of human origin weakly clumped the chimpanzee's blood cells, but this could be shown to be due to the natural heteroagglutinin in these sera. Pan's serum strongly clumped human red cells of groups A, B, and AB, but did not clump cells of group O. By adsorption tests, it could be shown that these reactions were due to anti-A and anti-B agglutinins in Pan's serum. Moreover, since adsorption with B cells lowered the titer for A_1 cells from 24 to 12 units, while adsorption with A_1 cells lowered the titer for B cells from 20 to 10 units, the chimpanzee's serum may also have contained anti-C agglutinins. At any rate, Pan's blood behaved serologically like human blood of group O.

M-N types. Pan's cells were strongly agglutinated by the single anti-M immune rabbit serum which was tried, but not by the anti-N serum used. Moreover, adsorption of the anti-M serum by the chimpanzee's cells destroyed its activity for human M cells, while adsorption by the chimpanzee's cells after treating the cells with ficin had no effect on the anti-M agglutinin. Thus, the M agglutinogen in the chimpanzee's blood cells, like the M agglutinogen in human blood, was damaged or destroyed by ficin, and presumably would be similarly affected by other proteolytic enzymes.

Rh-Hr types. With the newer techniques it was possible to demonstrate that Pan's blood contained variants of the two blood factors Rh_o and hr′. Tests for the other Rh-Hr factors, rh′, rh″, and hr″ yielded equivocal results, because the reagents available for these factors were not pure. That is, anti-rh′ and

TABLE 1

Demonstration of Rh_0 factor in chimpanzee "Pan's" red cells

ANTI-Rh_0 SERA	TITER (UNITS) FOR HUMAN RED CELLS [2]				TITER FOR CHIMPANZEE CELLS [2]	
	Rh_0 positive		Rh_0 negative			
	Unmodified	Enzyme treated	Unmodified	Enzyme treated	Unmodified	Enzyme treated
No. 1	90	100	0	0	0	90
No. 2 [1]	0	20	0	0	0	20
No. 3 [1]	0	2,000	0	0	0	1,600

[1] By the conglutination and anti-globulin techniques antisera no. 2 and no. 3 strongly clumped human Rh-positive cells, but gave negative or weak reactions with chimpanzee cells.

[2] The sera were titrated by diluting them with saline solution, and the red cells (both unmodified and enzyme treated) were suspended in saline solution.

TABLE 2

Reactions of anti-hr' sera with red cells of chimpanzee "Pan"

SERUM NUMBER		TITER OF SERUM FOR HUMAN CELLS				TITER FOR CHIMPANZEE CELLS	
		Type Rh_1rh		Type Rh_1Rh_1			
		Unmodified	Enzyme treated	Unmodified	Enzyme treated	Unmodified	Enzyme treated
No. 1	a. Unabsorbed	40	112	0	0	0	128
	b. Absorbed with unmodified type rh cells	0	20	0	0	0	7
	c. Absorbed with enzyme-treated type rh cells	0	0	0	0	0	0
No. 2	a. Unabsorbed	0	80	0	0	0	48
	b. Absorbed with unmodified type rh cells	0	3	0	0	0	2
	c. Absorbed with enzyme-treated type rh cells	0	0	0	0	0	0
No. 3		0	96	0	0	2	96
No. 4		0	12	0	0	0	12

anti-rh″ sera both contained Rh_0 blocking antibodies, while the anti-hr″ reagent contained an autoantibody.

Direct agglutination tests and conglutination tests on Pan's cells with all Rh_0 antisera tried gave negative or doubtful reactions. However, when the chimpanzee's blood cells were treated with ficin, they reacted with all anti-Rh_0 sera in the same way as enzyme-treated human Rh-positive cells (cf. table 1). Similar results were obtained with anti-hr′ sera. Thus, Pan's red cells contain an agglutinogen with blood factors behaving very much like Rh_0 and hr′. The fact that in earlier tests on chimpanzee blood, Wiener and Wade ('45) found all of 10 chimpanzees Rh_0 negative and hr′ positive, is readily understood, since the previous study was carried out by the classic tube agglutination technique using anti-Rh_0 agglutinating sera only, and a single unusually potent anti-hr′ agglutinating serum.

The results obtained again confirm the close serological relationship between chimpanzee and human blood — an example of biochemical evolution paralleling morphological evolution.

It is planned to continue and extend these investigations, using the newer techniques in order to test additional chimpanzees as well as other anthropoid apes and monkeys.

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NOTE ON THE NUTRITIONAL FACTOR IN HOWELLS' STUDY OF CONSTITUTIONAL TYPE

GABRIEL WARD LASKER
Wayne University College of Medicine

Numerous efforts have been made to achieve a useful synthetic description of individual physiques. As such descriptions of human constitution correlate somewhat with clinical and psychological variables, and as they are sometimes assumed to be inherent, it is important in the case of each new typology to attempt to evaluate the extent to which the diagnostic criteria are subject to change under the influence of changing diet and other vicissitudes of life as has previously been done for somatotype (Lasker, '47).

Howells ('52) has recently developed a technique for quantifying objective data on human constitution. In his approach to constitution, he has taken a series of 15 individuals considered to represent the three extremes in somatotype and has factored them in respect to 34 measurements. An orthogonal rotation, which is said to achieve the best solution, yields three significant factors. These factors are not the same as the three components of somatotype for, unlike the latter, they are not correlated. As a matter of fact, the analysis indicates that one of the factors actually encompasses two somatotype components, endomorphy and ectomorphy.

As the factors are based on the analysis of the intercorrelations of persons in respect to 34 measurements, it is clear that if the particular measurements which are of importance in the typology tend to be much influenced by milieu, then the types will reflect the effects of milieu as well as the inherent somatic tendencies of the individuals.

For this reason it may be of interest to consider Howells' factors in the light of data collected by Keys, Brozek, Henschel, Mickelsen and Taylor ('50) on the effects of partial starvation on some of the same measurements. On a diet restricted in caloric value for 26 weeks, 32 subjects showed an average weight loss of 24%. For the measurements included in both studies, the degree of reduction in the starvation study is indicated in table 1.

In Howells' scores by either of two solutions (orthogonal and oblique) those measurements which are most subject to change are highly positive in factor I, but not consistently distributed as to sign in respect to factors II and III (see Howells, tables 7 and 8). Partial starvation therefore would affect the score in factor I, but produce no great changes in the status of individuals in respect to II and III. However, factor I is quantitatively the most significant. In the orthogonal solution it contributes 4½ times as much to total variance as does II and 7 times as much as does III. It is clearly the prime element in the aspects of physique that have been differentiated by the measurements.

Of the effects of starvation, upper arm circumference shows the greatest, and upper arm circumference has the highest positive score in factor I. Thigh circumference shows the effects of dietary deprivation next most markedly, and mid-thigh circumference is second in positive score in Howells' factor I. Of those measurements represented in both studies, calf circumference and chest circumference are both highly influenced by partial starvation and score high in factor I. Chest breadth is somewhat less affected in either respect. On the other hand, stature and sitting height show very little change during partial starvation, and these measurements show negative scores for factor I. Biacromial diameter, which has virtually no score in factor I, is but moderately influenced by starvation.

Two measurements fail to follow the clear relation of all others in respect to score in factor I and reactivity to partial starvation: chest depth and bi-iliac diameter. Both show high scores in factor I but little change in the Minnesota starvation experiment. This discrepancy may in part result from differences in the technique of measurement. Different observers are known to fail to reach close agreement in measuring these two dimensions (Steggerda, '42; and Gavan, '50). The absolute values for Howells' series are not given, but as the data comprise one-half of that used by Dupertuis in 1950, the means published by Dupertuis may be taken as approximations. In chest depth and bi-iliac diameter the average figures for the control period of the starvation study are considerably smaller than Dupertuis' means and approach the figures Dupertuis gives for ectomorphs. Possibly the anthropometric technique in the Minnesota study was such as to compress or avoid soft tissues in these two measurements: the high reliability reported for bi-cristal diameter suggests compression, and measurement "to the vertebral column" in the anteroposterior di-

TABLE 1

Change during partial starvation and factor I score of measurements

	CHANGE DURING PARTIAL STARVATION IN PER CENT [1]	FACTOR I ORTHOGONAL ROTATED SCORE [2]
Upper arm circumference	— 24.5	2.53
Thigh circumference	— 19.4	2.03
Calf circumference	— 12.5	1.52
Chest circumference	— 7.5	1.68
Chest breadth	— 2.9	0.76
Biacromial diameter	— 2.3	— 0.04
Chest depth	— 1.7	1.58
Sitting height	— 0.7	— 0.43
Bi-iliac (bi-cristal) diameter	— 0.2	1.12
Stature	— 0.2	— 0.76

[1] Keys et al., vol. 1, tables 48 and 49, p. 134.

[2] Howells, table 7, p. 107.

ameter of the thorax suggests avoidance. On the other hand, all measurements of the control period in the Minnesota study are in the lower range of Dupertuis' series and resemble his ectomorphic means most closely.

It is possible that a general difference in physique between the acutely starved individual and one habitually undernourished during the growth period might be that the latter would develop both chest and ilium in proportion to the small abdominal size. This seems unlikely, however, for in migration studies which contrast the influence of long-standing dietary conditions during the growth period, chest depth seems to be a relatively stable measurement (Lasker, '46).

From the extreme negative scores given to chest depth and bi-iliac diameter in factor II in the orthogonal solution, and in factor III in the oblique solution, it seems probable that these measurements in particular manifest some property that accounts for the anomaly in the comparison with the starvation data. In any case, if further efforts at locating the reference axes for the factors of physique are attempted, it might be well to look for a solution tending to reduce the score for bi-iliac diameter and chest depth in the first factor.

In general, factor I represents the element of physique which is labile under changes in caloric intake. Howells says:

> "Factor I: As has been taken for granted, this contrasts girths with lengths or, roughly speaking, endomorphy with ectomorphy. Individuals positive in this factor are not necessarily short, or short-limbed, but rather girth, not length, is the prominent aspect of physique; and it is above all fullness of the upper arm and thigh which marks them: there is a taper effect to the fleshy limbs. At the opposite extreme, individuals negative on the factor are markedly skinny: limb lengths are prominent in their appearance, and especially the length of the first segments, upper arm and thigh. Both limbs and chest are poor in girth. There is a suggestion of greater prominence of facial features in these, but on the whole features of the head and face are little involved in this factor."

The conclusion suggested by the present analysis is that the description given by factor I is that of the nutritional state. It is interesting that it accounts for the major fraction of the total variance in a study of extreme somatotypes.

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* * *

THE FORSYTH FACE SIZE PROJECT — The Forsyth Dental Infirmary for Children announces the successful completion of the Face Size Project, an 18-month program in applied physical anthropology sponsored by the Army Chemical Corps.

Beginning in January, 1951, over 4,000 gas mask fitting tests were conducted, and as a result of the tests, and analysis of the anthropometric data, size schedules were set up, and size tariffs developed. In addition, two new tri-coordinate measuring ''Faceometers'' were designed and manufactured, and over 2,000 men, women and children were measured with these instruments. The data will be used in designing headforms, and will be available for other investigators.

The Face Size Project was directed by Dr. Stanley Marion Garn, who had had previous experience with the Chemical Warfare Service in 1942–43. Fitting tests were conducted by Helen J. Colby and Mary R. Crawford. The Faceometer program was organized by Dr. E. E. Hunt, Jr., assisted by George Maranjian. Deborah E. Younger was in charge of scheduling and programming. Harvard Graduate students in Anthropology participated in the ''Faceometer'' field tests.

PRESELECTION OF HEART DISEASE CANDIDATES.— The crux of preselecting men who will be prone to coronary heart disease is the learning of methods or techniques which will isolate those men from a putatively normal population. It is known that the man who is an endomorphic mesomorph is the most prone to coronary heart disease prior to the age of 40. While the somatotype is essentially the descriptive assessment of human structural variations, such a classification could be greatly enhanced by describing those further phenotypic expressions which are assessable only by biochemical analysis. This latter concept, which has been called "chemotype," appears to be a further step in the definition of the "normal" physique and its implications in disease have been discussed. By applying the principle of chemotype with somatotype and other variables such as hereditary factors and history, it may be possible to segregate the males most prone to coronary heart disease from those whose chances of having coronary heart disease are not as great or are unlikely. Accordingly, preselection will depend upon the recognition of those individuals who possess a combination of phenotypic expressions which under certain conditions are thought to be highly associated with the existence of either active or latent coronary heart disease.— Menard M. Gertler, Stanley M. Garn and Paul D. White. Young candidates for coronary heart disease. J. Am. Med. Assn., vol. 147, no. 7, October 13, 1951, pp. 621–625.

REVIEWS

BIOMETRICAL GENETICS. By K. Mather. 158 pp. Dover Publications, 1949. $4.50.

To someone who was not a geneticist or a physical anthropologist it might have seemed strangely paradoxical that the coming of the one science did not throw an immediate and immense light upon the other. Physical differences, particularly those of the skeleton so popular in the older anthropological literature, were obviously in the main due to heredity, so why could not genetics explain them? And the speciation occurring from ape to man — that matter of the continually discovered missing link — did not genetics set out to explain exactly that process? Yet here are 50 years gone by, and not a word of sense on the genesis of even so dear a topic as brachy- and dolichocephaly!

The trouble is, of course, that the large majority of physical characteristics, and probably of physiological and psychological ones also, show continuous variation, and not discrete classes. Mendelian genetics had at first very little to say about continuous variation; considering its immense importance in ordinary free-living animals (as opposed to the monstrous laboratory-supported stocks) and Darwin's own emphasis on the significance of small cumulative steps in evolutionary change, the silence was embarrassing, to say the least. As a matter of fact a great deal about continuous variation was being said, initially by Francis Galton, and later by Karl Pearson and the Biometric School, but the Mendelians thought this worse than silence, and stopped their ears against it. Likewise Pearson, to whom polemics came naturally, could in no circumstances allow any sense to Mendel's followers, and for many years the impasse remained complete. But, writes Mather, "the understanding of continuous variation awaited a fusion of the two methods of approach, the genetical and the biometrical, for each supplied what the other lacked. The one gave us the principles on which the analysis must be based; the other showed the way in which to handle continuous variation, the way of representing it in a form which made fruitful analysis possible" (p. 2).

The fusion was begun by E. M. East in the United States and by R. A. Fisher in England, and has latterly been furthered by several authors, perhaps most notably Mather himself and Sewall Wright. Mather's present book, however, is the first to present the genetics of continuous variation as a coherent and logical subject, and is of first-class importance for this reason. Particularly is this so for the physical anthropologist; we cannot any more ignore continuous variation by wishing it into a simple multimodality as did Davenport, nor by the declaration that physical anthropology is bunk and salvation lies in the mass-production of agglutinogens, useful and interesting though that certainly is.

Mather begins by describing the difference between *major genes,* those which have individually major effects on the organism and constitute, as he says, the backbone of the genotype, and *polygenes,* which have each very small effects, cannot be identified individually, occur in systems, are probably located for the most part in the chromosomal heterochromatin, and are responsible for continuous variation. The polygenes are ''the genes of fine adjustment . . . moulding the whole into the fine shape demanded by natural selection. They are the genes of smooth adaptive change and appear to be at the root of speciation. They are also the genes with which the plant and animal breeder is generally concerned in his endeavors to produce improved forms'' (p. 18). It is very much an open question whether there are two classes of genes — major genes and polygenes — or whether genes of all grades of effect between the dramatic and the minimal exist, but one can exemplify the difference dealt with here by saying that dwarf mice, dwarf rabbits and one sort of dwarf humans owe their characteristic to a major gene, while small mice, small rabbits and small humans owe theirs to a large number of polygenes.

Subsequently Mather goes on to discuss the relations of genotype and phenotype and the theoretical possibility of isolating the effect of a single polygene. (Those interested in the factor analysis of physique will find food for thought on p. 29, and the protagonists of discriminant functions ammunition on p. 32.) The seldom discussed but very important question of what scales to use to measure the effects of heredity and environment on a character receive a whole chapter. The scale should be chosen, Mather says, so that the genic effects are algebraically additive, and heritable and non-heritable variation if possible independent. Criteria for testing scales to see if they are in accordance with these desiderata are given.

Mather then discusses how to assess the degree of dominance of the genes in a polygenic system, how to test for linkage between the polygenes, how to estimate the number of effective factors concerned in the heredity of a character (effective factors not being quite synonymous with genes since one factor may comprise several closely linked genes) and the speed at which a continuously varying character can be altered under selection. Most of this discussion concerns the crosses and backcrosses obtained by breeding together inbred, nearly homozygous, lines because a much greater degree of exactitude is possible in these circumstances. The more general case of randomly breeding populations is also dealt with, however, and the background given, for example, of the rather well-known sib-sib correlation coefficient of 0.5 for many human measurements. Consideration of this part of the book shows — not that any physical anthropologist nowadays needs the demonstration — that comparative studies using the invaluable pure-bred lines of mammals available for example at the Roscoe Jackson Laboratory are of pre-eminent importance in the study of the heredity in man of all those physical and physiological traits which ordinarily distinguish one animal from another.

Professor Mather's book is not an easy one. Only somebody very much at home in both genetics and biometry will be able to follow all of it in detail at a first reading. But it is a book which is worth reading twice, and one which every physical anthropologist who aspires to an understanding of his subject will have to try to master, at least in part.

J. M. TANNER
Sherrington School of Physiology
St. Thomas Hospital, London

RADIOCARBON DATING. By Willard F. Libby, pp. vii + 124, 11 figures. University of Chicago Press, 1952. $3.50.

Libby suggests as a basic principle that "all atmospheric carbon dioxide is rendered radioactive by the cosmic radiation." Therefore plants, animals and sea water are rendered radioactive. As far as dating itself is concerned he recommends as follows, roughly in order: (1) charcoal or charred organic material, as heavily burned bone; (2) well-preserved wood; (3) grasses, cloth, and peat; (4) well-preserved antler and similar hairy structures; (5) well-preserved shell. In special chapters on "Preparation" and "Measurement of the Sample" the author discusses procedure in detail, and in Appendix A he lists the necessary special equipment and chemicals. The half-life accepted for radiocarbon is 5568 ± 30 years.

Of greatest importance to anthropology is the chapter on "Radiocarbon Dates." Representative areas and sites are from Mesopotamia and Western Asia, Western Europe, the United States, Mexico, South America, and "Other Areas" (Africa, Japan, Hawaii, Australia, Canada, Manchuria).

The research by Libby and his associates was carried on in affiliation with The Committee on Carbon 14 of the American Anthropological Association and the Geological Society of America. The work was financed by grants from the Wenner-Gren Foundation for Anthropological Research.

In a stimulating chapter on "The Significance of the Dates for Archaeology and Geology" Frederick Johnson, Chairman of the Committee, reviews Libby's dates and discusses their implications. He feels that the Carbon 14 technique, along with the archaeological, geological, and palynological sciences, will establish human and cultural chronology on a firm basis.

"Radiocarbon Dating" is mainly of interest to archaeologists, but many of the dates will be of import in physical anthropology.

Wilton Marion Krogman
Graduate School of Medicine
University of Pennsylvania

ERRATUM

In "The geological age of Pithecanthropus, Meganthropus and Gigantopithecus," by Dirk Albert Hooijer, in volume 9, number 3, of the Journal, there appears on page 272 the following sentence: "Dr. Teilhard de Chardin has always regarded it as more convenient, for China, to take the Villafranchian as Upper Pleistocene, . . ."

The last two words should read "Upper Pliocene."

STATISTICS OF DISCRIMINATION IN ANTHROPOLOGY

J. BRONOWSKI AND W. M. LONG
Central Research Establishment, National Coal Board, Cheltenham, England

One important step in anthropological research is to assign a new find to a known group, or to decide to which of several groups it is most nearly related. How does an anthropologist decide that a fossil bone is like a human bone, and unlike an ape bone? The shape and size of the bone may provide the answer at once: they may be quite different in the various species, and no mistake is possible. The evidence of the eyes, matched with previous experience, is enough. At other times this may not be so; the decision may be a close one, and mere visual judgments unreliable. In such cases, various dimensions of the individual may be measured, and compared with corresponding dimensions in the possible species, using statistical tests to estimate the importance of observed differences. The use of statistical tests in this way does not differ in principle from the judgment based on inspection coupled with experience. For in his appraisal of size and shape the anthropologist has in mind, consciously or unconsciously, some measurements which influence his decision, both singly and by their relation to one another, and these measurements seem to him to make it much more probable that the fossil belongs to one group rather than another. Statistical methods substitute a precise scale in terms of which these probabilities may be expressed.

One difficulty seems at first to present itself, however. How are the measurements which have been taken to be combined? A bone or a tooth is a unit, not a discrete assembly of independent measurements. To consider these measurements singly, as certain workers have done, is likely to be both

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inconclusive and misleading. It is inconclusive because nothing is decided when it is shown that, say, the height of an incisor tooth lies in the range of heights of gorilla incisors, and the breadth lies in the range of breadths of chimpanzee incisors; for this state of affairs may well be characteristic of a species different from both. And it will be misleading for a different reason, which is that all the measurements of such a set are correlated with one another: if the height of a tooth is above the average, its breadth and other dimensions are also likely to be above average, and the evidence, as one test is piled upon another, tends simply to repeat itself — the tests do not furnish entirely separate pieces of evidence, as at first sight they might appear to do.

The right statistical method must treat the set of variates as a single coherent matrix. This is the technique of multivariate analysis. As a case in point, consider for example the problem of deciding to which of two species a bone should be allocated. The multivariate technique can, ideally, be pictured to work as follows. The dimensions of this bone in every conceivable direction are measured, in many members of the two species; the averages of each dimension in each of the two species, the variance of each dimension, and finally the correlations between the dimensions are then calculated, giving a complete numerical picture of the size and the shape, and the manner in which they vary, in each of the two species. The next step is not to compare the dimensions one by one, nor even to single out those dimensions which, as single dimensions, are most critical. Instead, it is to construct *that function of the complete matrix of dimensions which is more critical than any other in discriminating* between the bone in one species and the other.

In practice, an endless set of measurements of a specimen cannot be taken. There would in any case be little point in doing so, for as more and more measurements are taken, after a while each fresh one inevitably bears an increasing physical resemblance to, and thus becomes more and more highly correlated with, some previous measurement. The additional information contributed by the new measurements, after a certain stage, becomes negligible. Likewise with the discriminating function: the sharpness of the discrimination, as more and more dimensions are incorporated in the function, rises more and more slowly. There is a limit to the sharpness of discrimination which can be obtained, a limit which represents the complete and ultimate appreciation of all the differences in size and shape of the bone, between the two species. In practice, provided the measurements are judiciously chosen, a small number of them is often enough to approach fairly nearly to this ultimate limit. The limited set then summarizes the whole configuration of measurements which makes the bone a unit.

As an example of topical interest, consider the problem of deciding whether a particular lower milk canine tooth belonged to a man or to a chimpanzee (Bronowski and Long, '51). Let us take 4 measurements, x_1 the height, x_2 the maximum anterior-posterior length of the upper half, x_3 the maximum breadth of the upper half, and x_4 the maximum anterior-posterior length at the level of the basal cingulum, all measured in millimeters. The discriminant function, as it is called, is of the very simple type

$$a_1x_1 + a_2x_2 + a_3x_3 + a_4x_4;$$

and the function we have obtained, using values of the means, variances and correlations of the measurements obtained from 40 human and 44 chimpanzee milk canines, is

$$X = x_1 - 7.49\,x_2 + 2.34\,x_3 + 4.70\,x_4 \qquad (1)$$

This has on the average the value

$$X = +17.6 \text{ for the chimpanzee,}$$

and has the average value

$$X = -5.0 \text{ for man.}$$

The standard deviation of X is 2.45, so that the range of X, taken at $2\frac{1}{2}$ times the standard deviation, is 6.1 on either side of the average. In order to classify a tooth, which is supposed to be either a chimpanzee or a human lower milk canine, x_1, x_2, x_3, and x_4 are measured in millimeters, and the measurements inserted in the discriminant function (1) above. If the result lies between $+23.7$ and $+11.5$ the tooth is chimpanzee; if between $+1.1$ and -11.1 it is human.

Sometimes the classification problem is wider, and it may be necessary to distinguish between several possible groups. Extensions of the method allow such problems to be dealt with in the same unambiguous and logically satisfying manner.

In the case of the fossil teeth 4 alternatives present themselves: man, the chimpanzee, the gorilla and the orang-utan. The main problem is, however, to distinguish between the human family on the one hand and the three anthropoid apes on the other, and as it happens the function (1) serves admirably for this purpose. Using further measurements from 40 gorillas and 34 orangs, we find that the range for the ape family as a whole is $+31.5$ to $+5.0$, and since the human range is as we have seen $+1.1$ to -11.1, effective discrimination is ensured.

As an application, we have calculated the values of this function for the Kromdraai and Taungs milk canines found in South Africa. The values are -2.6 and -7.9 respectively. They prove conclusively that these teeth do not belong in the ape categories. More than this, they fall precisely in the human range. It would be wrong to conclude positively that they are human teeth — there still remains the possibility that they come from another realm or species not considered in setting up the function. Even if this is so, however, the discriminant analysis indicates that the two fossils bear a closer resemblance (in a sense we shall describe later) to human than to ape teeth.

The discriminant function, as we have said, is designed for the especial purpose of sorting individuals which are initially assumed to belong to one of the families under consideration. In many cases this assumption is not open to question, for instance in the sexing of material from a known species. At other times it may not be certain that an individual does belong to one of the species under consideration. This brings us to another type of problem, that of deciding whether the size and shape of the individual are such as would be found in a certain species, allowing for the variation which is always present.

To meet this problem we again obviously need a technique which treats any set of measurements which may be taken as a coherent whole. If the dimensions are tested separately, some of them may show differences from the average for the group in question which, considered alone, would appear significant; others may not. When many tests are carried out, there is always an increased chance of finding a difference which, considered by itself, would be regarded as significant — on the principle that the more shots we make, the more likely we are to hit some target. It is very difficult to make allowances for this type of accident when the dimensions are correlated. Moreover, even when no single dimension shows a significant variation from its group mean, the total configuration may still be wrong. Multivariate methods attack this problem from a standpoint similar to that which led to the discriminant function. Ideally, one imagines a large number of individuals from the given species to have been measured in every conceivable direction. A function is then constructed which on the basis of these measurements expresses the *total* deviation in size and shape from the average for the species. In practice, of course, one uses a summary of size and shape based on a few major dimensions.

Using the same 4 dimensions, x_1, x_2, x_3, and x_4 which formed the basis for the discriminant function, we have constructed such a function for human milk canines. It turns out to be

$$S = (y_1 - 6.53)^2 + (y_2 - 11.96)^2 + (y_3 - 4.99)^2 + (y_4 - 1.91)^2 \qquad (2)$$

where y_1, y_2, y_3, and y_4 are quantities obtained from the x's by the relations[1]

$$\begin{aligned} y_1 &= x_1 \\ y_2 &= -0.345\,x_1 + 2.605\,x_2 \\ y_3 &= 0.078\,x_1 - 0.586x_2 + 2.337\,x_3 \\ y_4 &= -0.124\,x_1 - 1.159\,x_2 - 0.119\,x_3 + 2.050\,x_4 \end{aligned}$$

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[1] The significance of the y's is more fully explained in the appendix.

The function S, as we have already indicated, expresses in a numerical form the combined divergence of the set of measurements x_1, x_2, x_3, and x_4 from the average for human teeth, and as their configuration departs more and more from the human average, so does the value of S grow. In about one human lower milk canine in twenty, on the average, will it exceed the value of 6.0: and in only one in a hundred, on average, the value 8.4. For practical purposes we may take it that if an individual gives an S-value greater than 8.4 it is not human; if the S-value is between 6.0 and 8.4 the matter is open to doubt; but if the value is less than 6.0 then the configuration of the 4 measurements is such as would be found reasonably often in human teeth.

We selected three ape teeth at random from our basic sets, one chimpanzee specimen, one orang, and one gorilla: the S-values they gave were 62.7, 50.3, and 113.9 respectively. The Kromdraai and Taungs specimens on the other hand give S-values of 1.0 and 5.9 respectively, thus confirming their hominid character.

We must emphasize again the difference between the S-function (2) and the discriminant (1): the S-function is used to test whether the set of measurements is such as we would reasonably expect to find in human specimens, whereas the discriminant is designed to give an immediate decision to which family, man or the apes, the individual belongs, assuming that it does belong to one of them. In the former case, we are concerned with absolute probabilities, in the latter with relative probabilities.

Functions of the S-type can play a useful part in examination of the relations between different groups. As we have already seen, the value of S for an individual expresses, ideally, its divergence in size and shape from the average for the group to which the S-function relates — in a sense, the "distance" of the individual from the average or "center" of the group. A natural extension in dealing with a number of groups is to consider the distances between different groups, that is, the distances between their centers; it is sometimes possible to divide up an assembly of groups into constellations of the group, or "super-groups," the members of any one constellation being in general closer to one another, that is, resembling each other more, than members of different constellations. An interesting application of this technique to a number of Indian castes and tribes may be found in the Report on the United Provinces Anthropometric Survey (1941) by Mahalanobis, Majumdar and Rao, published in Sankhyā ('49).

The above very brief outline of some of the uses of multivariate analysis will, we hope, show that it is an instrument of considerable potential value in physical anthropology. It is interesting to note that the method was developed, in the first place, largely to meet problems in biological classification. It has been rather slow to take hold: perhaps co-operation between statisticians and anthropologists has not been as good as it might have been. Improvement in this respect should be of advantage to both.

To those who may wish to apply the technique, the best advice we can give is to call in the help of a trained statistician. The discriminant function method should be within the scope of those with a little knowledge of statistics (for details see R. A. Fisher, '48) but some of the other techniques need a fairly advanced training.

The authors are indebted to Prof. W. E. Le Gros Clark and Prof. J. T. Robinson for supplying them with the measurements of fossil, human and ape teeth which they have quoted.

APPENDIX

The purpose of this appendix is to show in simple terms what reasoning underlies the construction and use of the statistical functions mentioned in the text. We shall use the language of geometry.

In everyday solid geometry a point is specified by three co-ordinates. Let us take the 4 dimensions x_1, x_2, x_3, and x_4

of our milk canines as specifying points in a 4-dimensional space. Each tooth will then be represented by one point. If we have a large number of teeth of one species, say the chimpanzee, we shall find that the points corresponding to them tend to cluster around a certain central point or centroid, in such a way that the concentration or density of points in the space (that is, the number per unit volume) falls off with increasing distance from the centroid. Similarly, the points corresponding to human lower milk canines will cluster round another point, the human centroid. Suppose now that we have to decide to which of the two species a certain specimen should be allocated. A reasonable procedure would appear to be to find which centroid was nearer to the point representing the specimen, and to allocate it accordingly. Again, if we have to decide whether the 4 measured dimensions of a specimen are such that it can reasonably be taken to be human, we might use the distance of the corresponding point from the human centroid as a guide.

This idea of distance is indeed the key to the solution in each case, but first we have to make allowance for the fact that the points of each population or group are not symmetrically distributed around the centroid. The loci of equal point densities are not concentric spheres (as they would be if the distributions were similar in every direction) but ellipsoids, partly because of the correlations between the dimensions and partly because their variabilities are not all the same.

This difficulty is overcome by transforming the original variates to new ones which are uncorrelated and whose variances are all equal. The loci of constant point densities are then spheres, and the probability associated with any set of dimensions is directly related to the distance from the centroid of the point representing the set.

The significance of the equations in the text will now be more fully understood. They transform the original correlated variates x_1, x_2, x_3, and x_4 to uncorrelated variates y_1, y_2, y_3, and y_4, and the y's all have the same standard deviation. The S-function is simply the square of the distance of the point (y_1, y_2, y_3, y_4) from the point (6.53, 11.96, 4.99, 1.91), the latter being, in terms of the y's, the human centroid. The critical values 6.0 and 8.4 are the squares of the radii of spheres which enclose respectively 95% and 99% of the human population.

In this geometrical scheme the discriminant function appears as a device for the rapid comparison of distances: with two populations the value of the discriminant for a specimen shows at once which centroid is nearer to the point representing the specimen. This could also be done by calculating the value of the S-function for the specimen with regard to each population in turn, and, as one would expect, the discriminant function is related to the S-functions and can readily be deduced from them.

Since the distance from the centroid of a population to a point representing a specimen is in a sense a mathematical expression of the resemblance between the specimen and the average of the population, the discriminant affords a direct answer to the question of which of two groups a specimen resembles more closely. It remains, however, a relative measure — the resemblance in each case might be small. The S-function, being an absolute measure, is thus in a sense a more fundamental quantity.

The method of obtaining the transformation to uncorrelated variates may be found in an Appendix to the Report on the United Provinces Anthropometric Survey, already cited. The calculation is straightforward, if a little tedious, and once the transformation is obtained the S-function is easily constructed, being

$$\sum_{i} (y_i - \bar{y}_i)^2$$

the $\bar{y}_i$'s being obtained by substituting the means of the x's in the equations of the transformation. The critical limits for the S-function may be found from the fact that if the y's are expressed in standard measure, that is, are converted to variates with unit standard deviation by dividing by their original standard deviation, the S-function given by the standardized y's, for members of the population to which it relates, is distributed as χ^2 with degrees of freedom equal to the number of dimensions involved. This assumes that the basic distributions of the populations are multivariate normal, which with material of this kind is usually sufficiently accurate.

The discriminant function can also be obtained from the transformation equations, being simply

$$\sum_{i} d_i \, y_i$$

where the d_i's are the differences of the $\bar{y}_i$'s of the two populations in question.

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Rudolf Virchow.— To mark the 50th anniversary of the death of this great German scientist, the Armed Forces Medical Library in Washington has arranged an exhibit of his publications, supplemented by pictures and instruments. Included in the exhibit is a manuscript by Edwin H. Ackerknecht entitled ''Rudolf Virchow: Doctor, Statesman, Anthropologist.'' This first biography of Virchow in English is scheduled to appear in book form in 1953.

BLOOD GROUPS OF BRAZILIAN INDIANS[1]

A. M. PANTIN AND P. C. JUNQUEIRA
Department of Pathology, Cambridge University, and Instituto Oswaldo Cruz, Rio de Janeiro

This paper reports a study of the blood-groups of 73 Brazilian Indians. The region in which the blood samples were collected is that bounded by the rivers Batovi, Ronuro, Kurisevu and Kuluene, which are tributaries of the Xingú in the Matto Grosso. The tribes of this region are the following: Bakairi, Kalapalo, Nahukwá, Kuikuru and Matipuhy of the Cariba family; Waurá and Mehinaku of the Aruaque family; Auety and Kamaiurá of the Tupi family; and the isolated group Trumai.

Of these tribes, only two were accessible enough to provide blood samples which could reach the laboratory in suitable condition for testing. These were the Kamaiuras living near Camp Jacaré of the Fundação Brasil-Central, and the Kalapalos living in a fishing village near Camp Kuluene on the Kuluene river.

From Camp Jacaré we collected 35 blood samples, comprising 32 Kamaiurá (Tupi), 1 Trumai, 1 Waurá (Aruaque) and

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[1] This work was made possible by the generosity of the Brazilian Government, through Dr. Olympio da Fonseca, Director of the Instituto Oswaldo Cruz; Dr. Walter Oswaldo Cruz, Director of the Department of Haematology of that Institute; Dr. Modesto Donatini Dias da Cruz, Director of the Serviço de Proteção dos Indios; General Borges Fortes, Presidente da Fundação Brasil-Central; Orlando Vilasbôas, Chefe da Expedição Xingú-Roncador, and Dr. Pedro E. Lima. To all these, and to the Brazilian Air Force which supplied us with air-transport, we offer sincere thanks. We are especially indebted to Dr. A. E. Mourant, Director of the Blood Group Reference Laboratory, the Lister Institute, who not only provided the very valuable sera for our tests but calculated the gene frequencies from our results, and whom we would gratefully thank for his help.

1 Suiá (Gê). The first tests on these samples were set up in Rio de Janeiro 28 hours after bleeding, and completed two days later.

From Camp Kuluene, 104 samples were taken. Storms delayed our air-transport, and only 38 samples reached Rio in first-class condition for tests. These included 24 Kalapalos, 3 Nahukwá, 2 Matipuhy, and interbreeds as follows: 4 Kalapalo/Kuikuru, 2 Kalapalo/Nahukwá, 1 Kalapalo/Matipuhy, 1 Nahukwá/Matipuhy, and 1 Nahukwá/Kuikuru. All belong to the Cariba family. One sample only was from a cross between different families, viz. Kalapalo (Cariba)/Mehinaku (Aruaque). This second batch of tests was set up three days after bleeding and finished two days later.

MATERIAL AND METHODS

1. Cells. These Indians would not permit venepuncture, so finger-puncture was used. Blood from deep finger-stabs was allowed to drop into dry sterile tubes which were closed with sterile rubber bungs and placed upright in sealed tins immersed in cool water (about 10°C.) in thermos containers. These containers were transported by air from the Xingú to the laboratory in Rio de Janeiro.

For tests, only bright firm blood-clots in clear colorless serum were used. All clots not absolutely first-class were discarded. Each clot was shaken up in a little sterile 0.9% saline, the loose cells removed and washed, and an approximately 2% cell suspension, as judged by eye, reconstituted in sterile saline.

2. Sera. The sera used in these experiments were supplied by the kindness of Dr. A. E. Mourant, of the Blood Group Reference Laboratory, the Lister Institute. They were freeze-dried, and only made up as needed in sterile distilled water. They were specific for the antigens A, A_1, B, C, c, D, E, e, M, N, S, P, Lutheran, Kell, and Duffy (Fy^a). All except anti-M and anti-N, which were animal sera, were human sera of high titer, all except anti-Kell were absorbed to remove the natural agglutinins, and all except anti-Kell and anti-Fy^a were saline-agglutinating. Tests for Kell and Fy^a were done by the Coombs test (adding rabbit anti-human-globulin serum to the sensitized cells) and by the direct action of the testing serum on cells suspended in 20% human albumin. All the sera were used undiluted, except anti-A and anti-B, which were diluted to half-strength, lest these very powerful sera should lyse the cells.

3. Methods. The small-volume technique devised by Race was employed throughout, whereby equal volumes (approximately 0.008 ml) of serum and of 2% cell-suspension in saline are delivered from a marked Pasteur pipette, under direct vision, into clean dry precipitin tubes and kept for the necessary time at the appropriate temperature. Tests for A, A_1, and B were kept for two hours at room temperature. Tests for Rh antigens were incubated for 90 minutes at 37°C., and never centrifuged. Larger volumes were used in the tests for M and N. These, and tests for S, P and Lutheran were kept for two hours in a refrigerator at approximately 18°C. For Kell and Fy^a, 1 volume of 10% cell-suspension was incubated with 2 volumes of serum for 30 minutes at 37°C., and the cells, after 3 washings in saline, exposed to rabbit anti-human globulin serum (titer $\frac{1}{512}$, no zoning; used at $\frac{1}{20}$). Anti-Kell was also used in equal volumes with a 2% suspension of cells in 20% human albumin. When the test was ready, the contents of each tube were placed on a slide with minimal disturbance and read under the microscope ($\frac{1}{6}''$ objective). Controls of known cells were set up with each test. All the results were viewed by one person.

Further comments on the tests will be made under the separate headings. The gene-frequencies were calculated from our data by Dr. A. E. Mourant, using his method which is described in his papers on Basques and Indians (Chalmers et al., '49; Prasad et al., '49).

RESULTS

The ABO groups

The cells only, and not the serum, of the blood samples were examined. All these Brazilian Indians belonged to Group O. Of 27 *White controls*, the ABO groups were:

GROUP	NO. OBSERVED	FREQUENCY
A_1	4	0.2962
A_2	4	
A_1B	1	0.0741
A_2B	1	
B	3	0.1111
O	14	0.5186
	27	1.0000

The "Rhesus" blood groups

The sera used in these tests were of high titer and absolute specificity, and were used undiluted; the results were completely definite in all cases. An acute shortage of anti-e made it impossible to test all, or many, of the bloods for this antigen, and such tests as were made were disregarded in calculating chromosome frequencies. Apparently little information is thus lost. The phenotype ccddee (rr) was not observed in these Indians.

REACTIONS anti C D E c e	PHENOTYPE	COMMONEST GENOTYPE	NO. OBSERVED		TOTAL
			Cariba	Tupi	
+ + + +	CcDE	$R_1 R_2$	17	15	32
+ + — —	CCDee	$R_1 R_1$	11	11	22
+ + — +	CcDee	$R_1 R_0$	6	3	9
— + + + —	ccDEE	$R_2 R_2$	0	6	6
+ + + —	CCDE	$R_1 R_z$	4	0	4
— + — +	ccDee	$R_0 R_0$	0	0	0
			38	35	73

PHENOTYPE	COMMONEST GENOTYPE	NO. OBSERVED	NO. EXPECTED	FREQUENCY OBSERVED	FREQUENCY EXPECTED
CcDE	R_1R_2	32	28.14	0.4384	0.3855
CCDee	R_1R_1	22	25.06	0.3014	0.3433
CcDee	R_1R_o	9	5.62	0.1233	0.0770
ccDEE	R_2R_2	6	9.30	0.0822	0.1275
CCDE	R_1R_z	4	4.56	0.0548	0.0624
ccDee	R_oR_o	0	0.32	0.0000	0.0043
	Total:	73	73.00	1.0001	1.0000

Chromosome frequencies:

CDe	R_1	0.5859
cDE	R_2	0.2973
cDe	R_o	0.0657
CDE	R_z	0.0510
		0.9999

Of 27 White control bloods, the Rh groups were as follows:

PHENOTYPE	COMMONEST GENOTYPE	NO. OBSERVED
CCDee	R_1R_1	6
CcDE	R_1R_2	4
ccddEe	$R''r$	1
ccDE	R_2r (or R_2R_2)	6
ccddee	rr	4
CcDee	R_1r	4
ccDee	R_or	2
		27

The MNS groups

The interpretation of the MNS results presents a problem. The anti-M and anti-N sera used in these experiments were animal sera prepared at the Blood Group Reference Laboratory at the Lister Institute. Two different sera of the same specificity were used for every test, and these same sera had been in routine use at the Lister with satisfactory results against controls. In addition, these same sera gave the following results with 27 White European bloods used as controls in Brazil: 11 MN, 9 M, and 7 N, a distribution very close to the expected; and these results agreed with tests made on the same control bloods with commercial sera. The physical conditions of the tests were rigidly controlled. Nonetheless, the Brazilian Indian bloods gave a number of N-positives which were neither in accord with previous findings of other workers, nor statistically likely, and this feature recurred in the first 19 of a series of North American Indian bloods (Diegueños) tested with the same sera. When a different pair of anti-N sera was employed to test the remaining Diegueños, the numbers of N-positives dropped. On the other hand, a re-test of N-negative Brazilian Indians with the original sera still gave a negative result.

There are therefore three possibilities:

1. That the first pair of anti-N sera, which are notoriously difficult to make, contained non-specific agglutinins and gave a number of false positives.
2. That these sera contained in addition to anti-N an agglutinin against some unknown blood antigen which was picked up in different proportions of different populations.
3. That the high incidence of N in the peoples tested is real. In view of the reaction with the controls, and the fact that MM Indian bloods still gave a negative reaction on re-test with the doubtful anti-N sera, this is at least a possibility.

Without further experiment, it is impossible to determine which is the right answer, and the matter must be left there for the present.

The anti-M serum, and the human anti-S serum gave excellently clear-cut results.

Indians: MNS group

PHENOTYPE	NO. OBSERVED
MS	8
Mss	6
MNS	35
MNss	24
NS	0
Nss	1
	74[1]

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[1] One additional sample was tested for MNS, making 74.

Probably only one N, the rest being MN + M.

NO. OBSERVED		FREQUENCY	GENE FREQUENCIES	
M +	73	0.9865	M	0.8837
M —	1	0.0135	N	0.1163
	74	1.0000		1.0000

The evidence is insufficient to show how S genes are distributed between M and N.

Results of the White control tests were:

PHENOTYPE	NO. OBSERVED	FREQUENCY
MS	4	0.1482
Mss	5	0.1852
MNS	6	0.2222
MNss	5	0.1852
NS	1	0.0370
Nss	6	0.2222
	Total: 27	1.0000

The Lutheran antigen

This antigen was discovered in 1946 by Callender and Race ('46), who found its antibody in the serum of a man who had been many times transfused. This antibody has since been found, always as an immune response, on 5 further occasions. The Lu^a antigen is inherited as a Mendelian dominant character (Callender and Race, '46) and the phenotypes Lu^{a+} and Lu^{a-} can be recognized by the anti-Lutheran serum. Race and Sanger ('50) give the incidence of the Lutheran group in English blood as follows:

		Lu^{a+}	Lu^{a-}
Total no.	1373	105 (7.65%)	1268 (92.35%)

And the gene frequencies:

$$\begin{aligned} Lu^b &= 0.9610 \\ \text{and } Lu^a &= 0.0390 \\ \hline &\ 1.0000 \end{aligned}$$

Mainwaring and Pickles ('48) describe strongly and weakly reacting types of cells, and deduce the existence of three allelomorphic genes determining the nature of the Lu^a substance.

In the Brazilian Indians in the present study, 3 of the 12 positive results were weak, 9 strong.

	PHENOTYPE	NO. OBSERVED	FREQUENCY
Indians:	Lu^{a+}	12	0.1644
	Lu^{a-}	61	0.8356
		73	1.0000

Gene frequencies:
$Lu^b = 0.9141$
$Lu^a = 0.0859$
1.0000

Controls:

	NO. OBSERVED	FREQUENCY
Lu^{a+}	2	0.0740
Lu^{a-}	25	0.9260
	27	1.0000

The P antigen

A single human anti-P serum was used, and the test carried out at temperatures between 10°C. and 18°C. Positive results graded from large coarse agglutinates comprising all cells to weak agglutinates of a few cells among many unagglutinated ones. Four categories were established (#, +, [+] and weak) and a positive was recorded if any signs of general agglutination were present. But it was not always easy to interpret the results with this serum, as the border-line between a very weak positive and a negative was difficult to determine.

	PHENOTYPES	NO. OBSERVED	FREQUENCY
Indians:	P +	30	0.4110
	P —	43	0.5890
		73	1.0000

Gene frequencies: $p = 0.7675$
$P = 0.2325$

Controls:

PHENOTYPES	NO. OBSERVED	FREQUENCY
P +	14	0.5185
P —	13	0.4915
	27	1.0000

The Kell antigen

In 1946, the Kell antigen was identified by Coombs, Mourant and Race. Anti-Kell, in the "incomplete" form, had developed in the serum of a Kell-negative mother whose child was thought to have haemolytic disease. The Kell antigen was found to be inherited as a Mendelian dominant character.

Race and Sanger ('50) give the frequency of the Kell antigen in Caucasian bloods (White American and English) as follows:

	NO. OBSERVED	FREQUENCY
Kell$^+$	43	0.1017
Kell$^-$	380	0.8983
	423	1.0000

and the gene frequencies

$$k = 0.9478$$
$$K = 0.0522$$

Dunsford ('49), testing 566 Group-O bloods with a different anti-Kell serum, found the percentage of K positives to be 7.24, while Bertinshaw et al. ('50), using the same serum on a further random sample of 475 bloods, found 6.95% were K positive. The antibody in the anti-Kell serum used to test these Brazilian Indian bloods was "incomplete," and the serum, Group O, was not absorbed; as the Indians were all Group O, this was immaterial.

PHENOTYPES	NO. OBSERVED	FREQUENCY
Indians:		
K$^+$	17	0.2329
K$^-$	56	0.7671
	73	1.0000

Gene frequencies: $k = 0.8759$
$K = 0.1241$
1.0000

Controls:

PHENOTYPES	NO. OBSERVED	FREQUENCY
K$^+$	2	0.0740
K$^-$	25	0.9260
	27	1.0000

The "Duffy" antigen, Fy^a

The most interesting thing about these Indian bloods is that they all appear to lack the antigen Fy^a.

The example of anti-Fy^a used to test these Indian bloods did not agglutinate cells in saline, and the reaction on Fy-positive cells in 20% albumin was either extremely weak or negative. Each test was therefore done by the Coombs method. All 73 Brazilian Indian bloods gave negative results. The same serum used by the same method gave strong positives with 8 out of 16 White control bloods (Portuguese, English, French and German).

The blood-group antigen Duffy, or Fy^a, was discovered by Cutbush, Mollison, and Parkin ('50), who found the corresponding antibody in the blood of a man immunized by transfusion. The most complete published figures, those of Race and Sanger ('50), show that Fy^a is present in the blood of 65% of English people. Cutbush and Mollison ('50) have found it in 91% of persons from Northern Pakistan; Miller, Rosenfield and Vogel ('51) in 99% of Chinese and 26% of American Negroes; and Hartmann and Brendemoen [2] in 97% of Norwegian Lapps. It is an interesting fact, which may prove to be of some anthropological importance, that the antigen Fy^a has not been found in this group of Brazilian Indians.

SUMMARY

Blood from 73 pure-bred Brazilian Indians from the Matto Grosso (Kalapalos and Kamaiuras) has been examined for the antigens A, A_1, B, C, c, D, E, M, N, S, P, Lutheran, Kell, and Duffy (Fy^a).

All these Indians belonged to group O. All carried the antigen D. The phenotype ccddee (rr) was not observed. An unexpectedly high number of N-positives was found. Whether this finding was real or due to non-specific agglutination is discussed. Of these Indians, 58.9% were P-negative. The most interesting fact was that all the Indians tested were Duffy-negative. This fact may prove to be of anthropological importance.

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[2] Personal communication.

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Polynesian-Amerind Relationship.— The blood grouping figures favor neither Melanesia nor Micronesia [as the probable path of the ancestors of the Polynesian]. If no other facts were known they would almost compel one to look for another alternative. And the only obvious alternative, the very long landless ocean path from America, gains strong support from blood gene frequencies.

The serological similarity of these two groups which is not unsupported by likenesses in other criteria suggests that the Polynesian and the Amerind have a common dominant component. Further, this component, perhaps somewhat differentiated during centuries in the widely diverse environments of the Americas, could have reached Oceania at any time with the aid of prevailing ocean currents. That ocean currents have had a major influence on the migratory journeyings of the Polynesian has gained much support in recent years following the successful enterprise of Heyerdahl and the involuntary voyages of two other groups quoted by Avias, 1949.

These three remarkable ocean voyages took place within a space of 12 months. How many similar sailings have taken place over the centuries we have no idea, but we know that long ocean voyages taking advantage of ocean currents and prevailing winds were well within the capabilities of the seafaring early Polynesian.

Avias ('49) has suggested that the Polynesian has resulted from admixture of an Amerind component with a proto-Melanesian group which had arived in Polynesia before the Amerinds. He described the latter group as a negrito-ainoid complex with possibly a dash of Papuan. Our results suggest that the proto-Melanesian component must be weak in Polynesia, as it has left little serological evidence of its presence in the Maoris.

Nevertheless there appears to be much to indicate that another race reached some of the islands of Polynesia.

This group, though perhaps outnumbered by the "Amerinds," may well have been responsible for some of the similarities of culture and language that have been quoted in support of an eastward course for the Polynesian. It is also likely that since his original settlement in the central Pacific the Polynesian has, on occasion, driven westward into both Melanesian and Micronesian territories and possibly even into Indonesia.

In conclusion it is submitted that the serological evidence presented in this paper strongly supports a Polynesian-Amerind relationship, making it probable that the islands of Polynesia have been settled largely by migrations from continental America. Prevailing ocean currents and other factors suggest the coast of Peru as the starting point of such migrations.— J. J. Graydon. Blood groups and the Polynesians. Mankind, vol. 4, no. 8, March, 1952, pp. 329–339.

SKELETAL MATURATION OF WEST AFRICAN NEGROES

J. S. WEINER AND V. THAMBIPILLAI[1]

Anthropology Laboratory, Department of Human Anatomy, University of Oxford and Anatomy Department, University of Malaya

THREE FIGURES

INTRODUCTION

Despite the fact that for some 15 years standards of skeletal maturation for American children of white stock have been available (Flory, '36; Todd, '37), very few studies of other racial groups have been carried out.

In the present investigation the "skeletal age" of Negro boys from W. Africa has been studied using Todd's Atlas of Skeletal Maturation ('37) and also that of Greulich and Pyle ('50). Despite the smallness of the sample this seemed worth while as no systematic study of African Negro skeletal growth appeared to have been made previously and moreover the ages of the boys were reliably known. After the completion of our statistical analysis, there appeared the report by Mackay ('52) on a much larger group of East African Negro children with results very similar to ours.

METHODS

The material comprises the x-rays of the wrists and hands of 60 Accra school boys, all but two of the ca tribe, ranging in age from 9 to 20 years. The radiograms were obtained during

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[1] Our thanks are due to Dr. Geoffrey Tooth for his unfailing help and interest in this study. We are deeply grateful to Mr. R. Morley Jones for his assistance with the statistical analysis of the results.

Acknowledgment is due to the University of Malaya for a travel-study grant to one of us (V.T.).

1946 in the course of investigations for the Colonial Social Science Research Council by Dr. Geoffrey Tooth who also provided the information as to certified ages, the heights and weights and other particulars. In the assessment of the skeletal age of these 60 boys most emphasis was placed on Todd's standards since for these the statistical data (mean and standard deviation) are available for large numbers of American subjects from infancy to 17 years. This information is provided in the introductory text of Greulich and Pyle's Atlas (p. 31) and refers to boys of the Brush Foundation Study.

For each of the 60 Negro boys the two observers T and W made two assessments according to Todd's procedure. Assessments were made also using Greulich and Pyle's Atlas by both observers but in this case only once for each subject. The radiograms were identifiable only through their serial numbers and were examined in a completely random order by the two observers working independently. In making comparisons between Americans and West Africans it was thus possible to take account of the observers' variability and, in the case of Todd's procedure, also of the effect of duplicate estimates.

RESULTS

In table 1 is given the individual data for the 60 boys — their certified ages and the assessments of their skeletal ages as determined by American white standards.

Assessment of skeletal age on Todd's standards

The mean chronological age for the group of 60 boys as a whole is 14.56 years and this may be compared with the mean skeletal age obtained by Todd's procedure:

Mean estimated skeletal age (years)

Observer T		*Observer W*		*Both observers*
1st estimate	*2nd estimate*	*1st estimate*	*2nd estimate*	*both estimates*
13.22	13.12	13.16	13.07	13.14

(standard error for all means 0.039)

TABLE 1

Skeletal age assessment (years) of West African Negro school boys

CHRON. AGE GROUPS	X-RAY NUMBER	CHRON. AGE	TODD'S ATLAS				GREULICH AND PYLE'S ATLAS	
			Observer T		Observer W			
			1st est.	2nd est.	1st est.	2nd est.	Obs. T	Obs. W
years								
8.6–9.5	5	9.16	7.25	7.25	7.25	7.25	7.13	6.45
9.6–10.5	51	9.75	6.25	6.25	6.25	6.25	5.46	5.62
	59	9.83	9.75	9.75	9.75	9.75	8.08	8.80
	56	9.91	7.75	7.75	7.25	7.75	7.54	6.35
	48	10.08	9.75	9.75	10.25	9.75	8.23	9.33
10.6–11.5	10	10.58	9.25	9.75	9.75	9.25	8.08	8.30
	8	10.75	10.75	11.25	10.75	10.75	9.35	11.60
	9	10.91	10.25	9.75	9.25	9.75	7.73	8.90
	49	11.00	10.75	10.25	10.75	10.25	9.88	10.20
	55	11.16	8.25	8.25	8.25	8.25	7.46	7.70
11.6–12.5	57	11.75	11.25	10.25	10.25	9.75	9.19	9.20
	60	12.00	11.75	11.75	11.75	11.75	9.30	9.44
	58	12.41	7.75	8.25	8.25	8.25	6.27	6.85
	22	12.41	11.75	11.25	11.75	11.75	10.15	11.40
12.6–13.5	54	12.83	13.25	13.75	13.25	12.75	11.81	12.78
	6	12.83	11.75	12.25	11.25	11.75	11.98	11.87
	4	12.91	11.25	10.75	11.25	10.75	10.69	12.65
	25	13.00	12.25	12.75	12.25	12.25	12.56	12.20
	33	13.16	10.75	10.25	10.75	10.25	8.31	9.67
	50	13.25	10.25	11.25	10.75	10.75	9.65	9.00
	31	13.33	10.75	10.75	10.75	10.75	9.73	11.10
	29	13.41	11.25	10.25	10.75	10.25	10.65	10.28
13.6–14.5	36	13.58	11.75	11.75	11.75	11.25	11.75	12.64
	52	13.83	12.75	12.25	13.25	12.75	12.53	13.08
	13	13.91	11.25	10.25	10.75	10.25	9.46	10.00
	46	14.00	12.25	12.75	13.25	13.25	12.80	13.91
	7	14.16	10.75	11.25	10.25	10.75	9.58	10.00
	24	14.16	15.25	15.25	14.75	14.75	14.71	15.16
	53	14.25	8.75	8.75	8.25	8.25	7.77	7.93
	27	14.25	14.25	14.25	14.25	14.75	13.76	14.38
14.6–15.5	32	14.58	12.75	13.75	13.75	13.25	13.05	13.77
	26	14.75	12.25	12.75	12.75	12.25	11.75	13.67
	19	14.75	15.25	14.75	14.75	14.75	15.00	15.00
	3	14.75	14.25	13.75	14.25	13.25	13.34	12.96
	11	14.91	15.25	14.75	14.75	14.75	14.94	15.31
	12	14.91	12.75	12.75	12.75	12.75	12.85	12.87
	28	14.91	13.75	13.75	14.25	13.75	12.50	13.08
	2	15.25	13.75	13.75	13.75	13.75	13.63	13.94
15.6–16.5	23	15.91	15.25	15.75	15.25	15.25	15.62	15.05
	34	16.00	17.25	16.25	16.25	16.25	16.00	16.35
	17	16.00	17.25	17.25	16.75	16.75	16.25	17.00
	42	16.08	15.75	15.75	15.75	15.75	15.45	15.22
	41	16.08	14.25	14.25	14.25	14.25	13.91	13.66
16.6–17.5	15	16.75	15.75	15.25	15.25	15.25	15.37	14.30
	20	16.83	15.75	15.75	16.25	15.75	15.37	15.21
	38	16.91	15.75	15.75	15.25	15.75	15.30	15.29
	45	17.08	16.75	16.25	16.75	16.25	15.87	16.50
	14	17.50	14.75	14.75	14.75	14.75	15.00	13.77
	30	17.50	16.75	16.75	16.25	16.75	17.25	17.00
	40	17.50	16.25	15.75	16.25	16.25	15.80	16.71
	44	17.50	16.75	16.25	16.25	16.25	15.84	16.50
17.6–18.5	16	17.83	17.75	16.75	17.25	17.25	16.00	17.00
	21	17.83	15.75	15.75	16.25	15.75	15.88	15.82
	18	18.16	16.75	16.75	17.25	17.25	17.25	18.00
18.6–19.5	37	18.58	17.25	16.25	16.75	16.75	16.75	17.00
	47	18.58	17.25	16.75	17.75	17.75	17.00	17.50
	1	18.91	16.25	15.75	16.25	16.25	15.64	15.82
	43	19.33	17.25	17.25	16.75	16.75	17.25	17.00
	39	19.50	17.25	17.25	17.25	17.25	17.25	16.92
19.6–20.5	35	20.16	18.75	18.75	18.75	18.75	18.00	17.50

These findings suggest that the stage of skeletal development reached by the Negroes is about 16 months, on the average, behind their chronological age, or the stage of skeletal development of American boys of the same chronological ages, since the Todd skeletal ages accord closely with the chronological ages of the boys in the Brush Foundation Study (Greulich and Pyle, '50). A detailed comparison between the West African and the American groups is made below. The means presented above indicate also some variation between the duplicate assessments and this is examined more closely in the following analysis of variance of the 240 estimates of the skeletal ages:

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	
Subjects	59	2246.2614	38.0722	
Assessments	3	0.8864	0.2955	$F = 3.23$
Error	177	16.1761	0.0914	$P < 0.05$

The small variation between the separate assessments is attributable in this case not to differences between the two observers but to the small (and just significant) decrease in the estimate of skeletal age with repetition of the assessment. This implies that estimates made at different times by different observers may differ by a small but significant amount. In making a comparison between the Africans and Americans a small correction on this score is strictly necessary but its magnitude is much too small to affect the results.

Assessment of skeletal age on Greulich and Pyle's standards

The means on this assessment are as follows:

Mean estimated skeletal age (years)		
Observer T	*Observer W*	*Both observers*
12.55	12.88	12.72

The two observers are in agreement in the finding of a lower skeletal age for the Africans by this as by the previous method. They concur also in finding that these standards yield a lower mean skeletal age than does Todd's procedure. This finding may be more apparent than real for the two observers differ significantly in their assessments by Greulich and Pyle's procedure. The mean difference is 0.330 years and the standard error of the difference ± 0.0937 so that $t = 3.52$ and $P < .001$ (degrees of freedom 59). This difference between the observers has no relation to the subjects' ages.

When the two methods are compared the differences between them may be shown to bear some association with age. Thus the 60 pairs of comparisons between the mean values of the 4 estimates by Todd's method with those of the two by Greulich and Pyle's method for the 12 age groups yield the following variance analysis:

SOURCE OF VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	
Different age groups	11	9.2603	.8418	$F = 3.126$
Residual	48	12.9257	.2693	$P < .01$

The following table shows further that the Greulich-Pyle procedure underestimates skeletal age rather more at lower than at higher ages.

Age group (years)	9	10	11	12	13	14	15	16	17	18	19	20
Difference (years) Todd-Greulich	.46	.95	.86	1.49	.44	.22	.13	.32	.21	.09	.09	1.00
S.e. of difference	.52	.26	.23	.26	.18	.18	.18	.23	.18	.30	.23	.52

It is of interest also that in the hands of the present two observers the Greulich-Pyle assessments are more variable than the Todd values, for the standard error per observation is 0.513 and 0.312 respectively which differ significantly ($F = 2.88$, degrees of freedom 59 and 177).

It seems then that the Greulich and Pyle assessments, while confirmatory of the result given by Todd's procedure, may leave more room for observers' error.

Comparison of African and American boys

The x-ray film of the carpal region given by Todd as the standard for any particular age represents that most frequently found in American boys of the Brush Foundation Study at that age, so that the average standard skeletal age corresponds closely to the average of the chronological age group. Greulich and Pyle ('50, p. 31), using larger numbers than were available to Todd, give data showing clearly how closely the mean chronological age (for each of 24 age groups from infancy to 17 years) corresponds to the mean skeletal age. These estimates of the skeletal age of the large numbers of American boys in the age groups 9 to 17 years have been compared with the African skeletal ages in the following table:

TABLE 2

Comparison of skeletal ages of African and American boys by Todd's method

CHRONOL. AGE GROUP	AMERICAN BOYS			AFRICAN BOYS			DIFFERENCE	S.E. OF DIFFERENCE
	Mean skel. ages	No.	S.D.	Mean skel. ages	No.	S.D.		
years	*months*			*months*				
8.6– 9.5	113.90	160	9.00	87.00	1	..	26.90	..
9.6–10.5	125.68	177	9.79	100.50	4	21.04	25.18	10.55
10.6–11.5	137.32	154	10.09	117.30	5	12.20	20.02	5.25
11.6–12.5	148.82	165	10.38	125.62	4	20.18	23.20	10.12
12.6–13.5	158.39	175	10.44	136.50	8	11.92	21.89	4.29
13.6–14.5	170.02	163	10.72	144.75	8	25.57	25.57	9.08
14.6–15.5	182.72	124	11.32	164.81	8	10.41	17.91	3.82
15.6–16.5	195.32	99	12.86	189.30	5	12.75	6.02	5.85
16.6–17.5	206.21	68	13.05	190.88	8	7.64	15.33	3.13

In all of the 8 age groups, except that at 16 years, the lower skeletal age of the Africans differs significantly from that of the Americans.

In figure 1 the results, including the findings by the Greulich-Pyle procedure, are summarized graphically.

DISCUSSION

The sample is admittedly a very small one but the results are quite consistent over the whole age range and indicate that the skeletal development of these Gold Coast boys is retarded by comparison with Americans of white stock of similar age. The difference in skeletal age between the two groups amounts to about 20 to 24 months at chronological ages from 9 to 14, falling to less than 18 months at chronological ages 15 to 17 (table 2). There is thus some suggestion that the Negro skeletal maturation may be accelerated to some extent towards puberty but the smallness of the sample and its "cross-sectional" nature precludes any finality on this point.

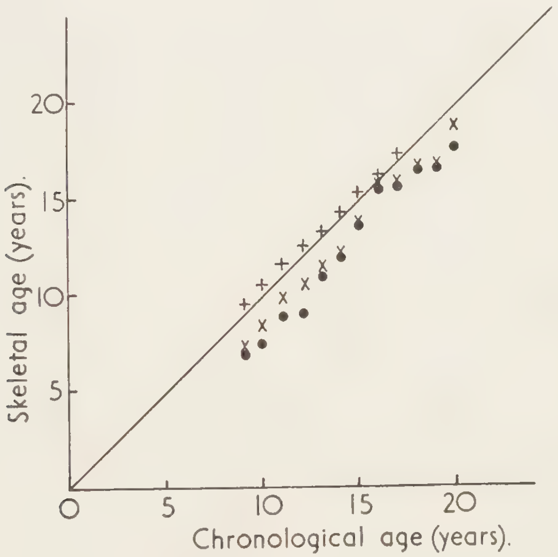

Fig. 1 Skeletal vs. chronological age.

+ American boys (Todd standards).
x West African boys (Todd standards).
● West African boys (Greulich and Pyle standards).

Our results in general accord very well with those recently published by Mackay ('52) for East African Negro children as may be judged from his statement: "If comparisons are made between the age standards for the Wadigo (East Africa) children and those of American children produced by Flory and Wingate Todd, it will be seen that after the 5th year the difference becomes more and more marked, until by the age of 10, the Africans are one and one-half to two years behind and that by the age of 17½ this difference has still not been made up."

The West African boys are behind American boys of the Brush Foundation Study not only in their skeletal maturation but also in bodily size. Despite the small numbers of the Negro sample, there is no doubt that the Negro boys are markedly shorter and lighter at all ages as may be seen in figures 2 and 3. The irregularity in the Negro results at age 16 is in all likelihood due to sampling. The American boys are ahead by an equivalent of at least two years, a period comparable to their advance in skeletal maturation. Comparison with the boys of the Iowa Study (Meredith, '35) or that of Gray and Ayers ('31) confirms this large disparity in body size of the Negro boys.

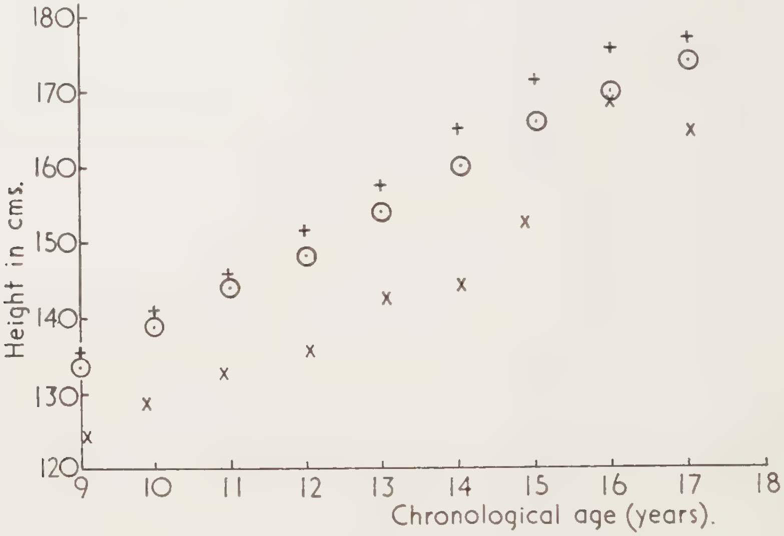

Fig. 2 Standing height in relation to chronological age.

+ American boys (Brush Foundation Study).
⊙ American boys (Iowa Study).
x West African boys.

To explain with any finality why the Africans should display a less advanced skeletal development and smaller body size as compared with the Whites is difficult at present but the problem merits some discussion if only to emphasize the necessity in future investigations of attempting to distinguish between the possible causes. The obvious possible causes of

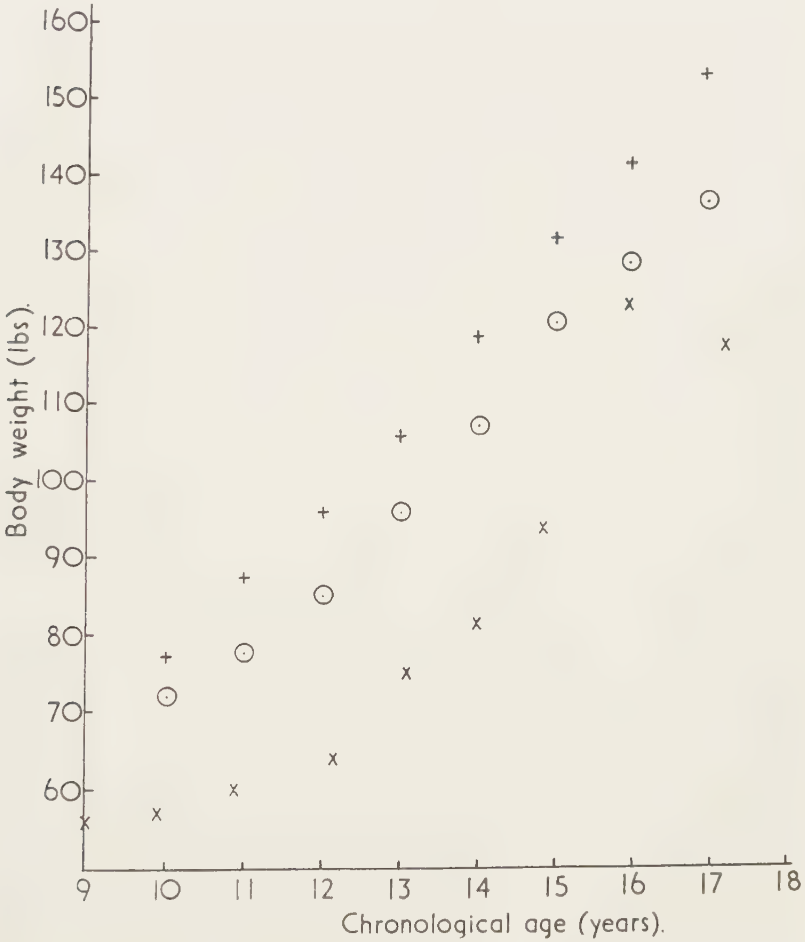

Fig. 3 Body weight in relation to chronological age.

+ American boys (Brush Foundation Study).
⊙ American boys (Iowa Study).
x West African boys.

the difference between the two groups are nutritional, climatic and "racial." The influence of these and other factors on bodily development has been reviewed by Palmer and Ciocco ('41), and Stuart ('46). Stuart concludes that there is satisfactory evidence that undernutrition may retard maturation as well as physical growth. There seems to be rather little specific information on the relation of nutrition to skeletal maturation; some data indicating a likely effect of malnutrition in retarding skeletal development is provided in papers by Mack and Urbach ('49), Abbott et al. ('50) and Greulich ('51). That the physical difference between the Africans and the Cleveland children may thus be due in part to nutritional status is hardly to be doubted. The influence of their economically well-favored status on the body size of the boys of the Brush Foundation Study is clearly revealed by comparison with other American groups (Meredith, '35). The fact that the Accra school boys were attending one of the better boarding schools is not a reliable guide to the financial position of the parents. It is the usual practice for many members of the family to subscribe to send one boy to school, and even though 34 of the 60 boys had fathers in "Government service," this section of the community is said to be often in debt. The likelihood that these boys subsisted on relatively poor standards of nutrition during school holidays and in infancy cannot be ignored. One should also remember that in this region there is a high incidence of worm infestation while yaws and malaria are endemic.

Important as the nutritional factor undoubtedly is, yet the possible effect of a tropical climate in retarding skeletal growth and maturation, as Mills ('37, '50) has emphasized, cannot be discounted, neither in the case of the present investigation nor in Mackay's East African studies. In both instances the samples are drawn from a population living in truly tropical conditions. That tropical dwellers are usually of smaller body weight seems certain from an extensive geographical survey of body dimensions carried out in this laboratory (D. F. Roberts, in course of publication) but it is often stated quite categorically (e.g., De Castro, '52) that this is due to malnutrition. The possible adaptive significance of smaller body weight in hot climates in man has been given all too little attention. The smaller the body size the greater the relative surface area and Schreider ('50) has provided some data to show that the ratio of surface area to body weight increases towards the tropics. Where the production of body heat is directly dependent on body weight, as in various forms of work, the relatively greater surface area of the smaller individual would appear to carry some advantage in heat dissipation.

The operation of environmental factors of nutrition and climate do not of course exclude the possibility of genetic or "racial" differences in body weight and height or skeletal development. That in the present case we are dealing with differences in some degree environmentally induced, is further supported by Todd's ('36) opinion that in the same socio-economic circumstances Negro skeletal maturation in the United States is no different from European, and by observations such as those of Thieme ('50) on the Porto Rican population, where the large effect of socio-economic status on the body size of Negroes is well demonstrated.

Although we have spoken of the Africans as "retarded" by American white standards, a later or slower maturation may well be the phenotypically "normal" expression for their particular ecological circumstances. "Normal" standards would therefore be different for example from those of Americans of Caucasian stock. The smallness of the present sample and the possible complication of poor nutritional status obviously preclude the use of the present findings as a basis for such standards.

SUMMARY

Assessments have been made of the skeletal maturation of 60 Negro boys from West Africa, in the chronological age range of 9 to 20 years. By comparison with American stand-

ards the skeletal ages have been found to be delayed by an average of 16 months. The significance of this finding is discussed in relation to nutritional and climatic factors.

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THE BODY SURFACE AREA OF ESKIMOS AS DETERMINED BY THE LINEAR AND THE HEIGHT-WEIGHT FORMULAS

KAARE RODAHL[1]

WITH THE TECHNICAL ASSISTANCE OF JAMES EDWARDS, JR.

Department of Physiology, Arctic Aeromedical Laboratory, Fairbanks, Alaska

In discussing the basal metabolic rates in Eskimos, it has been suggested by several workers (Wilson, '45; Bollerud et al., '50) that the DuBois-DuBois height-weight formula, when utilized for the calculation of body surface area, may not be applicable for the Eskimo. This would introduce an error in the calculation of the results, which might partly explain the high basal metabolic rates usually observed in the Eskimo.

Small errors in the DuBois-DuBois height-weight formula, as applied to the Chinese and Japanese, have been reported (DuBois, '36). Necheles and Loo ('32) found the height-weight formula on the average 4.4% higher for the Chinese, and concluded that it was not necessary to alter the constants of the DuBois-DuBois height-weight and linear formulas for the Chinese. Waddell, Han and Chen ('28) observed in their series of 74 Chinese subjects a maximum variation between the two methods of 4.2%, the average being ± 1.25%, which is about the same as is found among Europeans and Americans. Stevenson ('28, '37) measured the surface of 10 Chinese using both the linear and height-weight formulas. The totals obtained by adding the surface of different parts according to the linear formula agreed closely with the results calculated according to the height-weight formula. This, however,

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[1] The author is greatly indebted to Dr. E. F. DuBois for his valuable criticism and advice throughout this study, and for his personal supervision during the initial measurements. He is also indebted to Lt. Col. A. Karstens, Commanding Officer of the Arctic Aeromedical Laboratory, for his valuable support.

was due to a fortuitous compensation between errors in different regions, and Stevenson has suggested a new set of constants for the linear formula when applied to the Chinese.

Takahira and Kitagawa ('24) made careful measurements of 10 adult Japanese men and found the best results by using a constant for the height-weight formula of 72.46, which is 0.9% higher than the original constant. The method they used for their measurements is described as follows: "The hair was first cropped close to the head, and then paper was pasted on the nude body with water and then with 1% starch paste, and dried with a fan. When there was formed a thin filament over the body, it was removed. The surface of this was measured with a number 4 Amsler's planimeter. The planimeter gives a 0.2% error which was neglected, but the shrinking of the paper was corrected."

The surface areas for the different parts of the body in Whites, Chinese and Japanese are given in the authoritative monograph by Edith Boyd ('35) in table 1, pp. 6 and 7.

The actual body surface area of Eskimos has never been measured previously. Since surface area determined from weight in kilograms (Wt.) and height in centimeters (Ht.) according to the DuBois-DuBois height-weight formula: Area (Sq. Cm.) $= \text{Wt.}^{0.425} \times \text{Ht.}^{0.725} \times 71.84$ is widely used as a standard of reference in the calculation of basal metabolic rates in Eskimos, it is necessary to ascertain whether or not the height-weight formula is applicable to the Eskimo race.

MATERIAL

In conjunction with a series of comprehensive studies of the metabolism in Eskimos, the body surface has been carefully measured by the DuBois linear method (DuBois and DuBois, '16) in 53 Eskimos from 5 different locations in Alaska. The present paper compares the results of these determinations, in which the total body surface is obtained by adding 7 partial surface areas computed on the basis of lengths and circumferences of these parts, with the results obtained by the height-weight formula for estimation already described.[2] All of the subjects, with two exceptions, were believed to be full-blooded Eskimos; 42 were males and 11 were females. The age ranged from 16 to 53 years, but the majority were between 20 and 40 years of age, the average being 28. The study included the following groups of Eskimos: One group of 10 Eskimos came from Barter Island on the north coast of Alaska. A second group of 24 Eskimos lived at Gambell on St. Lawrence Island in the Bering Sea. A third group of 4 subjects came from Anaktuvuk Pass in the middle of the Brooks Range, 3000 feet above sea level. The 4th group consisting of 8 Eskimos, were obtained from Kotzebue, and a final group of 7 came from Selawik on the west coast of Alaska.

For the purpose of comparison, the surface area was measured in 7 Whites of similar age by the same investigators using identical technique.

METHOD

A great variety of methods are available for the measuring of surface area. Most of these, such as the covering method, geometric method, photographic method, integrator method or the electrical method, are complicated and time consuming. For the purpose of this study, the much simpler DuBois linear formula (DuBois and DuBois, '16), which only requires approximately 15 minutes for each subject, was chosen. This method, which determines the average length and circumference of each part of the body, is probably the most accurate of the simplified methods of determining the surface area. Furthermore, we had the privilege of receiving personal guidance and instruction from Dr. E. F. DuBois, himself, for the measurements of our first subjects during his visit to our laboratory in February, 1951.

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[2] For extended version of this article, with individual measurements, order Document 3712 from American Documentation Institute, 1719 N St., N. W., Washington 6, D. C., remitting $1.00 for microfilm (images one inch high on standard 35 mm motion picture film) or $3.90 for photocopies (6 × 8 inches) readable without optical aid.

The method involves division of the surface of the body into 7 regions: head, arms, hands, trunk, thighs, legs, and feet, and the estimation of the area of each by measuring characteristic lengths and circumferences, multiplying and correcting by means of factors. The constants for arms, thighs, etc., when multiplied by the measurements of one side, give the surface of both sides. The total of the 7 parts when added represent the total surface area.

In all cases the subjects were measured lying on a flat table, stretched out with their legs close together. The arms and legs on the right side were always measured. Both the height standing and the length lying were carefully recorded for comparison, but for the final calculations only the height was used, since the height-weight formula is based upon the body height measured standing.

In some cases repeated measurements were made of the same subjects, as a check on the technique, and the results showed very close agreement. Furthermore, in a few cases measurements were made of the same Eskimo subject both in the winter and in the summer. The variations were found to be insignificant.

RESULTS

From table 1 it is observed that the average difference between the linear and the height-weight formula was ± 1.3% for the Eskimos as against ± 1.5% for Whites reported by DuBois ('16). The maximum difference was 3.8% in the Eskimos as against 5.0% in the Whites reported by DuBois ('16). In 12 cases out of 53, the height-weight formula and the linear formula gave identical results. On an average the height-weight formula gave results which were 0.9% higher than the results obtained by the linear method in the Eskimos. This is within the limit of accuracy claimed for the height-weight chart (DuBois and DuBois, '16) and is therefore insignificant. In the 7 Whites measured by the same technique by the same investigators, the height-weight formula gave results which were on the average 1.9% higher than the linear formula (table 1). The maximum difference in these Whites was 3.6% and in one case the two methods gave identical results.

TABLE 1

Body surface area in Eskimos and Whites

	ESKIMOS			WHITES
	Average of 53 males and females	Average of 11 females	Average of 42 males	Average of 7 males
AGE, WEIGHT, AND HEIGHT				
Average age, years	28 (16–53)	30 (21–53)	28 (16–47)	28 (19–68)
Average weight, kg	63.2 (43–91)	63.2 (50–91)	63.2 (43–74)	78.0 (62–103)
Average height, cm	164.0 (151–178)	157.1 (151–163)	165.8 (152–178)	176.9 (168–185)
Average length, cm	165.8 (153–180)	158.3 (153–166)	167.8 (155–180)	178.9 (170–187)
SURFACE AREA				
Ht.-Wt. surface area, m^2	1.68 (1.33–1.96)	1.63 (1.48–1.96)	1.69 (1.33–1.88)	1.95 (1.71–2.27)
L.-Wt. surface area, m^2	1.70 (1.39–1.98)	1.64 (1.48–1.98)	1.72 (1.39–1.91)	1.96 (1.73–2.28)
Linear surface area, m^2	1.67 (1.36–1.96)	1.61 (1.47–1.96)	1.68 (1.36–1.88)	1.91 (1.72–2.19)
DIFFERENCE IN PER CENT				
$\frac{\text{Ht.-Wt.} - \text{Lin.}}{\text{Linear}} \times 100$	+0.9 (−2.2 +3.8)	+1.1 (−0.5 +2.7)	+0.8 (−2.2 +3.8)	+1.9% (±0.0 +3.6)
$\frac{\text{Lt.-Wt.} - \text{Lin.}}{\text{Linear}} \times 100$	+1.8 (−1.8 +5.0)	+1.6 (−0.5 +4.1)	+1.8 (−1.8 +5.0)	+2.7% (+0.5 +4.2)

In Eskimo women (11 subjects), the average difference between the height-weight formula and the linear formula was + 1.1%. If the length was used instead of height, the difference between the two formulas was + 1.6%. In the 42 Eskimo men, the average difference between the height-weight and the linear formulas was +0.8%, and the difference between the "length-weight formula" and the linear formula was + 1.8%.

Table 2 presents the surface area of the different parts of the body in Eskimos and Whites for comparison. In table 3 these findings are expressed in per cent of the total surface area. From this table it appears that in the Eskimo a comparatively larger part of the total body surface area is represented by the trunk, head, and hands than in the Whites, while the rest of the body constitutes a smaller portion of the total surface area in the Eskimo as compared with the Whites. These differences are small, however, and appear to have no practical significance.

TABLE 2

Surface area (M^2) of different parts of body in Eskimos and Whites

PART OF BODY	ESKIMOS			WHITES
	Average of 53 males and females	Average of 11 females	Average of 42 males	Average of 7 males
Head	0.1180 (0.1057–0.1354)	0.1136 (0.1057–0.1328)	0.1191 (0.1058–0.1354)	0.1222 (0.1079–0.1365)
Arms	0.2274 (0.1777–0.2860)	0.2091 (0.1898–0.2553)	0.2322 (0.1777–0.2860)	0.2823 (0.2369–0.3749)
Hands	0.0904 (0.0680–0.1232)	0.0760 (0.0680–0.0866)	0.0942 (0.0699–0.1232)	0.0996 (0.0918–0.1060)
Trunk	0.6013 (0.4609–0.7673)	0.6008 (0.5180–0.7673)	0.6014 (0.4609–0.6841)	0.6516 (0.5510–0.7462)
Thighs	0.3174 (0.2473–0.4043)	0.3211 (0.2845–0.4043)	0.3164 (0.2473–0.3630)	0.3763 (0.3206–0.4541)
Legs	0.2071 (0.1727–0.2511)	0.1944 (0.1742–0.2267)	0.2104 (0.1727–0.2511)	0.2467 (0.2093–0.2879)
Feet	0.1084 (0.0902–0.1602)	0.0970 (0.0902–0.1060)	0.1114 (0.0983–0.1602)	0.1316 (0.1135–0.1500)
Total	1.67 (1.36–1.96)	1.61 (1.47–1.96)	1.68 (1.36–1.88)	1.91 (1.66–2.19)

TABLE 3

Surface area of different parts of the body in per cent of the total surface area

PART OF BODY	ESKIMOS			WHITES
	Average of 53 males and females	Average of 11 females	Average of 42 males	Average of 7 males
	Average age: 28 years	Average age: 30 years	Average age: 28 years	Average age: 28 years
Head	7.1	7.1	7.1	6.4
Arms	13.6	13.0	13.8	14.8
Hands	5.4	4.7	5.6	5.2
Trunk	36.0	37.3	35.8	34.1
Thighs	19.0	19.9	18.8	19.7
Legs	12.4	12.1	12.5	12.9
Feet	6.5	6.0	6.6	6.9

CONCLUSION

On the basis of the reported data, it may be concluded that in the Eskimo the difference in the body surface area as estimated by the height-weight formula and by the linear formula, is within the limit of accuracy claimed for the height-weight chart. The height-weight formula is therefore applicable also to the Eskimo, and it is evident that the use of this formula does not cause any significant error in the calculation of the basal metabolic rates of Eskimos.

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PHYSICAL TYPES OF FRENCH WEST AFRICA.— The Coniagui, Bassari and Badyaranke resemble others Negroes of the savannah measured heretofore. Here in a transitional territory, where two zones of vegetation meet, there lives a group of populations which is also transitional: not as large in size as certain of the savannah Negroes but larger than those of the forest. They might well (as Richard Molard thinks) approximate an ''old Sudanese'' type which would formerly have occupied a larger area, rather than an actual ''open country,'' classic ''neo-Sudanese'' type. It would be the Coniagui-Bassari in particular which approached the ''old Sudanese,'' the Badyaranke being already more frankly ''neo-Sudanese,'' according to their size. Furthermore, in explaining the differences between these two types, it is perhaps not necessary to invoke mixture, as does Molard, or in any case not mixture alone. It has been noted that populations may, in the course of their history, exaggerate certain of their traits: Eskimo populations becoming more ''Eskimized,'' for example, as one goes toward the eastern end of their territory. Why should not the Negroes have become ''Negritized'' little by little (the concept of ''Negro'' is to begin with a concept created by the Whites, corresponding to an imaginary Negro, as different as possible from a White: there is acutally no population combining the darkest skin, the longest limbs, the broadest nose, the thickest lips, etc.,) by the play of genetics (aided by a marked consanguinity stemming from the smallness of the isolate), of selection (which has a potent role in these backward areas without doctors), of the influence of the environment, and so forth? — Monique de Lestrange. Contribution a l'étude de l'anthropologie des Noirs d'A. O. F. II. Anthropométrie de 1023 Coniagui, Bassari, Badyaranké et Fulakunde de Guinée Francaise. Bull. Soc. d'Anth., Xe ser., vol. 1, 99–136. 1950. (Translation.)

THE EFFECT OF WEIGHT-TRAINING ON PHYSIQUE[1]

J. M. TANNER
Sherrington School of Physiology, St. Thomas's Hospital, London

ONE FIGURE

That human muscles respond to hard work by growing bigger is a matter of common belief, particularly in gymnasia. The hypertrophied heart is a commonplace of the autopsy room; and through the haze of anecdote one can discern dimly that the skeletal muscles of some types of athlete, when subjected to prolonged and intense strain, may grow to more than their usual size. One recalls, also, the versatility of the uterus in this matter. Nevertheless, astonishing though it may seem, we have practically no quantitative information on the degree to which human skeletal muscles can be made to grow by exercise. We do not know how much muscle an ordinary man can put on, nor whether he will keep any or all of it when he ceases training. We do not know the relative potentiality for growth of the various muscles, nor whether this varies, so far as the exercise stimulus is concerned, from childhood to maturity. We are in the dark, even, as to whether the growth of a child's muscles or other tissues is affected, temporarily or permanently, by muscular exercise per se;

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[1] Besides my debt to the subjects of the experiment and to Mr. John Barrs and Mr. Henry Atkin, I wish to thank Dr. G. A. Maw for doing the creatinine estimations, Mr. N. A. Barnicott for the loan of an anthropometer, Mr. M. J. R. Healy for a critical reading of the manuscript and Mr. R. H. Whitehouse for his skilled assistance throughout the whole investigation. I am most grateful to Professor Henry Barcroft for his interest in and support of the experiments; and I wish to acknowledge with thanks the William Hyde Award for 1948 from the Research Board for the correlation of Medical Science and Physical Education, which helped defray expenses.

there seem to be no studies of the matter that really stand up to criticism, nor indeed would it be at all easy to plan one that did. Neither do we know — and the matter is bitterly disputed in body-building circles — what sort of exercise is best for causing muscles to grow, and whether this or some other stimulus is the more appropriate for the different objective of increasing strength.

Consequently it seemed of value to make a limited study of some aspects of this subject when the chance was offered through the goodwill of two friends in the weight-lifting and body-building world. The present study owes its inception and completion to John Barrs, a member and adviser of the British Weight-lifting team at the 1948 Olympic Games, and Henry Atkin, a well-known instructor of weight-lifters and body-builders. The effect of weight-lifting over a period of 4 months has been investigated on 10 healthy young men, all of whom were trained by Mr. Atkin at his gymnasium, The Viking Barbell Club, during the spring and early summer of 1949. Changes in the physique of these young men have been followed by taking some 20 anthropometric measurements approximately every three weeks, and, at the same time, standardised photographs from which 19 other measurements were obtained. Two complete measuring sessions a few days apart were held before the training began in order to establish the errors of measurement, and this has enabled the changes observed in each individual to be assessed statistically as to whether each was likely to be ascribable merely to such errors (that is, to "chance") or not. Four months after the subjects had stopped their training they were again measured, so that what had happened to any significant increases in measurement — and there were gains of up to 3½ cm in arm circumference, for example — could be seen.

There were in addition two subsidiary, more technical, objectives of this experiment. Photogrammetric anthropometry (Tanner and Weiner, '49; Tanner, '51a), as the precise measurement of the human physique by photography is called, is a relatively new technique, and it seemed useful to test its accuracy under the very stringent conditions provided by this experiment, where we were trying to record the small changes in body measurements occurring during the three-weekly periods between tests. Secondly, there is the question as to whether severe muscular exercise of this sort is sufficient to lead to erroneous ratings of anthroposcopic somatotype; and whether the post-exercise anthropometric somatotype obtained from Sheldon's tables (Sheldon, '40) would give an erroneous or anomalous figure, and, if so, how great the error would be.

MATERIAL

The subjects were volunteers from amongst the students attending the Walthamstow Technical College. About 40 students volunteered for the experiment, and 16 were selected, a deliberate effort being made to include men of different somatotype groups. Of these 16, however, 5 early deserted us voluntarily or due to circumstances beyond their control, leaving 11. These 11, interestingly enough, were with one exception remarkably similar in physique; two of them were of somatotype 263, one 253, one 254, one 245, one 352, one 353, one 354, two 452s and one solitary 631. Needless to say, faced with this competition, the 631 also gave up before the end, and has had to be excluded from our mean figures.

This is therefore a study of weight-training in 10 healthy, rather highly mesomorphic young men and its results should not be generalised beyond this physique group. Their ages ranged from 18 to 25, with one subject in each of the age groups 18, 19 and 20. Growth in most dimensions in most men has ceased by this age, and if not ceased altogether is very slow. No growth in stature occurred in any of the subjects during the experiment. The subjects mostly lived in lodgings, and even superficial enquiry showed that their diet was not optimal, largely for financial reasons. However, it was certainly not grossly restricted. Most of the students took regular ordinary exercise throughout the course of the experiment, the majority playing some game or other on Saturdays.

METHODS

The *training period* extended from the beginning of April to the end of July, with an hour's weight-training three evenings a week (usually between 4 and 6 P.M.). There were breaks of 2 to 5 training days at Easter (two weeks after the start of training) and of one to three training days at Whitsun (in mid-training). Inevitably there were also individual missed attendances, and some subjects left for home at the end of the summer earlier than others. Thus the actual attendances numbered, for the several individuals, 23, 23, 23, 24, 29, 33, 36, 37, 39 and 44. The individual missed attendances up to the time each subject left varied from two in the case of subject G to 9 for subject L, the average being between 4 and 5.

The *training schedule* followed that usually used by Mr. Atkin in his body-building and weight-lifting school. During the first 6 weeks of training the exercises were as follows: two hands barbell curl, lateral raise lying, triceps stretch seated, and straight arm pullover, each for 3 sets of 10 repetitions each, and the deep knee bend for 3 sets of 20 repetitions each. The details of these exercises and illustrations of how they are done can be found in Mr. Atkin's book ('50); a more general account of body-building and weight-lifting is that of Barrs ('47). After this 6 weeks' preliminary period, training became more intense, consisting of 3 sets of 10 repetitions of standing curl with dumbells, press behind neck seated, flying exercise, and short radius pullover; 4 sets of 10 repetitions of bent-over curl and pullover to arm's length; 3 sets of 20 repetitions of deep knee bend; and calf stretching with toes on block for a period of 5 minutes. The poundages used on each exercise were chosen for each individual by the instructor, and increased as the subjects increased in strength.

The *body measurements* shown in table 1 were taken at the times detailed below on each subject. The usual methods of measurement were used; the precise details for those other than the circumferences will be found in a previous publication (Tanner and Weiner, '49). The limb circumferences were taken with a tape, checked against the anthropometer before and after the experimental period. No springs were used on the tape ends, but what is believed to be the same, slight-to-moderate, pressure was applied on each occasion. The upper arm circumference was measured at a level halfway from the tip of the acromion to the top of the head of the radius, this level being marked with a silver nitrate line so that it did not fade between measurement sessions. The maximal circumference of the forearm was taken. The thigh circumference was taken by placing the top edge of the narrow tape into the angle formed by the skin of the buttock and the back of the leg, that is, the point from which photogrammetric measurement LTU_1 (Sheldon, '40) is taken. The calf circumference measured was the maximum, relaxation being achieved by having the subject put his left foot onto a footstool, bearing his weight on the right. The upper arm contracted was the maximum circumference with the biceps contracted and the arm flexed (as in the advertisements). Forearm and thigh contracted were made with the subject contracting both extensors and flexors so far as possible, and calf contracted with his weight on the left foot. All these measurements were taken on the left side, and in a plane at right angles to the long axis of the limb.

Photogrammetry was routinely carried out at the same time as the measurements were taken. The subject was photographed in front, side and rear views at a 10 m lens-centre of turntable distance, with a 10 cm heelplate-centre of turntable distance using the 35 mm camera technique previously described (Tanner and Weiner, '49), with Panatomic X film. Enlargements on waterproof base paper were made such that the 1 m distance in the centre of rotation plane became 120 cm on the enlargements, the usual limits for accepting a print, 119.6 and 120.4 cm, being used. The 19 measurements were taken off the enlargements with photogrammetric calipers; they were the dimensions given by Sheldon ('40) plus two added in our previous study. The upper arm diameter ATU was taken at the same level as the circumference, using the same mark from one time to the next. The subjects were posed in the standard way for photogrammetry and somatotyping (Dupertuis and Tanner, '50), so that the pose differed as little as possible from one session to the next.

The *urinary excretion of creatinine* was determined at each session on a 24 hour collection, taken on nearly every occasion in the 24 hours ending on the morning of the measurement day. Most of these collections were taken from Sunday to Monday mornings. The creatinine was estimated by Dr. G. A. Maw, to whom I am most grateful for this data, by the method previously described by him (Maw, '47).

The *measurement sessions* numbered 7. There were two (A and A^1), two days apart, before any exercise began; the duplication was to give an estimate of measuring error. B was after 4 days' work, C after a further 12 days' work with the Easter break before the 12; D after a further 9, with Whitsun midway between C and D; and E after 9 more. Finally in early early December, after the summer holidays and most of another term had passed, a last session Z was held to find out what had happened to the measurements 4 months after the end of the weight-training period; none of the subjects trained on weights during this 4 months. Creatinines were not done at session Z.

All measurements on the body were taken by the writer and recorded by an assistant. No measurement could be remembered from one session to the next, and the assistant only intervened to request a remeasurement if it seemed probable that a mistake in reading the instrument had been made. As to the pictures, the whole 65 were arranged in order consecutively from time A, subjects 1 to 10, B1 to 10, etc., Z1 to 10, and then their distinguishing marks were covered up. The pictures were then completely mixed by reference to a table of random numbers; the top one being given the first number between 1 and 65 in the table, the second the second number, and so on. They were then measured in this random order. Thus, as the photographic technique was too similar for it to identify a particular session, no observer bias enters into these measurements.

Somatotyping. Sheldon's ('40) anthropometric tables strictly speaking cover only the age group 17 to 20 inclusive, but the error involved in applying them to students aged 21 to 22 is small and we are in any case not concerned here with small somatotype differences so far as pre-training somatotypes go. Consequently all the subjects except nos. 1 and 10, who were 23 and over, were somatotyped anthropometrically using Sheldon's tables with the alterations for the 10 m distance previously given (Tanner and Weiner, '49). The remaining students were somatotyped anthroscopically, with reference to a key file. Anthropometric somatotyping was done on pictures A and E, to see if any difference occurred.

MEASUREMENT CHANGES

Changes in total group

The individual differences in reaction to this training were fairly large, depending somewhat on the individual's physique, somewhat on his environmental circumstances before and during the experiment, and to a very considerable extent on his willingness to work hard at his training. It is consequently not entirely satisfactory to report the results in generalities, but we can conveniently start that way. We will subsequently analyse the effect of training on three particular individuals.

In figure 1 are shown the means of those measurements with significant average change during training. Significance was judged by a simple t-test of the mean gain on any given occasion relative to its standard error. If on more than one occasion the mean departed from zero by over twice its standard error, a significant average change was said to have occurred; this would lead to a probability level of considerably less than 5%. The means are for 8 individuals, not 10, since two were not present for remeasuring at Z.[2] The zero starting-point from which the subsequent gains or losses are computed is the average of the A and A^1 values. The gains are plotted simply, as there was no apparent association of gain and initial value in this restricted batch of data. Photogrammetric measurements have been multiplied by the appropriate factor, 8.33 for most measurements, to bring them to exact life size.

Statistical significance in the case of individual changes was assessed by whether or not values subsequent to the control figure depart from the control by more than twice the standard error of measurement s_{meas}. The latter has been calculated from the figures on 70 subjects reported previously for the same technique (Tanner and Weiner, '49), and is given for the various measurements in the first row of table 1.[3]

In the second column of table 1 is given the standard error of measurement as estimated from the 10 subjects of the present study. These estimates are, of course, considerably less precise than those in the first column, and are reported only to show that the technique remained approximately the same in accuracy from the earlier study to the present one. There are only 7 differences between the pairs of estimates significant at the 5% level; two of these are on the heavily skewed distributions of subcutaneous tissue, and one or two would in any case be expected on a chance basis for a series of 25 comparisons. In general the s_{meas} for the 10 subjects is a little larger than that for the 70, however, and this is perhaps to be expected, since the period of time elapsing between measurements was a few minutes for the 70 subjects and a couple of days for the 10. Thus the s_{meas} for the 70 represents the variability due solely to measurement whereas for the 10 it represents this plus such minor changes in morphology as may occur in two days.

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[2] Three sets of values (out of 56) have had to be interpolated, since one individual was absent at B, and two others absent at E. Interpolations were made by drawing smooth curves by eye through the whole sequence of values for the individual concerned. This will give a more valid impression of the mean effect than would plotting the 7- or 5-individual averages at B or E; it is essential to maintain what in growth studies would be referred to as complete longitudinality, as otherwise inter-individual differences distort the average curve. (See Tanner, '51b.)

[3] The variance of measurement, s^2_{meas}, is one-half of the variance of the difference of duplicate measurements; in this case the variance of the difference includes errors in measuring the pictures both between and within observers and errors of posing on two occasions by a single observer.

The largest departing of the values at times B, C, D, and E was initially compared with s_{meas} from the 70 subjects, and, if it exceeded twice s_{meas}, the rest of the values were examined to see whether this departure was an isolated event or whether a fairly uniform trend was shown. If an isolated event, it was not taken as significant unless a re-check of the picture for posing and measurement showed it to be clearly correct, and unless it was over three times s_{meas}. In almost every case, however, a uniform trend *was* present, and this immediately raises the significance (i.e. lowers P) greatly, since, assuming no trend, the probabilities of each departure are independent and all contribute to the overall probability of the series of changes not being due to chance, that is to unavoidable errors in measurement. Consequently, in what follows, it may be taken that when an individual's change is said to be significant, the probability level is considerably below .05 and the non-chance effect is practically certain. A more sophisticated statistical approach would probably pick out still smaller changes as not really ascribable to chance.

It should be remembered that these two tests of significance, of the mean gain and of the individual's gain, test two fundamentally different hypotheses. The first is that the population of young men of which our 10 are a sample would, on testing, show a gain (or loss) on average; the second that this particular individual did in fact gain (or lose) tissue during the experimental period. The latter seems the more interesting, particularly as our sample is from a population restricted in both age and physique.

Figure 1 is divided into three blocks, with the vertical scales the same for each. The blocks deserve attention decreasingly from above downward; the top is the most im-

TABLE 1

Changes in various bodily dimensions of three young men during 4 months' weight-trainin (B,C,D,E) and 4 months after its cessation (Z)

DIMENSION	s_{meas} (70 SUBJS. TANNER AND WEINER)[1]	s_{meas} 10 SUBJS. PRESENT STUDY	PRE-TRAINING VALUES; AVERAGE OF MEASUREMENTS AT A AND A′		
	Stand. error is		Subj. G	Subj. M	Subj. C
	0.085 s_{meas}	0.22 s_{meas}			
Weight (lb.)	. .	. .	157.5	119.5	161.5
Stature (mm)	3.7	. .	1785	1658	1738
Circ. upper arm loose	3.5	5.4	292	246	277
Circ. upper arm contract.	. .	2.7	315	284	299
Circ. forearm loose	2.6	2.7	272	242	270
Circ. forearm contract.	. .	2.8	278	253	280
Circ. thigh loose	3.2	7.2	543	481	551
Circ. thigh contract.	. .	6.9	548	485	553
Circ. calf loose	3.0	2.0	372	336	376
Circ. calf contract.	. .	2.8	365	331	370
Subcut. upper arm front	0.64	0.78	5	4	6
Subcut. upper arm back	0.75	0.90	10	6	9
Subcut. subscap.	0.81	2.1	11	9	12
Subcut. suprailiac	0.66	1.8	10	7	12
	mm on life-size			mm life-size	
FB$_1$	. .	1.3	153	148	149
FB$_2$	1.1	1.6	142	133	137
NTap	0.7	0.7	114	102	117
NTt	1.1	1.6	128	113	120
TT$_1$	2.6	3.2	232	216	224
TTnip	2.8	3.4	256	230	242
TT$_2$	3.0	4.2	204	189	234
TT$_3$	2.6	2.5	238	220	258
TB$_1$	4.6	5.9	380	337	347
TB$_2$	0.6	2.2	288	249	285
TB$_3$	1.7	3.9	351	328	345
ATU	1.6	2.4	121	106	114
ATL$_1$	2.5	1.8	87	79	94
ATL$_2$	. .	2.3	68	62	71
ATL$_3$	0.9	1.4	64	58	69
LTU$_1$	2.2	1.6	189	158	179
LTU$_2$	2.2	1.6	138	112	136
LTL$_1$	0.8	1.5	122	109	118
LTL$_2$	0.7	1.5	54	52	58

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[1] Calculated from data on 70 men (Tanner and Weiner, '49). Photogrammetric dimensions include variability due to different observers; living body dimensions relate to one observer only.

TABLE 1 *(continued)*

Changes in various bodily dimensions of three young men during 4 months' weight training (B,C,D,E) and 4 months after its cessation (Z)

SUBJECT G					SUBJECT M					SUBJECT O			
B	C	D	E	Z	B	C	D	E	Z	B	C	D	E
mm above average of two pre-training determinations					mm above average of two pre-training determinations					mm above average of two pre-training determinations			
0.5	6	4.5	2.5	3.5	1	5	5	7	7.5	—0.5	—6	—7.5	—7
—1	—3	2	2	—1	0	—3	—3	—1	—3	4	—3	—2	0
5	25	17	19	5	6	33	32	34	19	19	0	7	8
8	22	23	26	14	7	29	22	29	18	10	11	6	12
7	6	13	12	7	—1	18	13	15	1	7	5	2	2
2	8	12	5	4	6	13	11	12	5	9	4	2	3
6	21	11	2	4	—3	12	12	24	16	2	—25	—18	—19
8	30	15	5	12	7	16	18	28	22	12	—22	—15	—19
—3	2	—5	—4	0	2	—4	—6	—2	—4	3	—6	—11	—10
1	—3	—4	—6	—1	—3	—6	—2	—3	—2	3	—10	—14	—16
1	1	1	0	1	0	0	0	1	0	0	0	—1	—1
—1	1	—1	—1	—1	1	0	1	2	2	0	—1	—1	0
0	2	1	0	1	2	3	4	4	4	0	—1	—1	—1
—2	2	2	—2	1	3	5	5	4	8	1	—3	0	—2
mm gain, life-size					mm gain, life-size					mm gain, life-size			
1	—2	0	—2	—2	—1	—2	—1	0	3	0	—2	—1	—4
—3	—3	—2	—2	—1	—1	—3	—4	—1	0	—3	—3	—3	—3
—1	—1	1	—2	0	0	—1	2	0	3	—2	—2	0	–
—2	1	1	—2	2	2	3	5	6	2	0	2	1	—4
—1	—1	—2	—5	2	1	3	—1	—1	8	—4	—5	—11	–
1	3	—6	—1	2	—4	1	—2	—1	9	7	—2	7	–
—2	—5	—7	—9	2	—7	—4	—3	—6	12	—12	—33	—25	–
—4	—4	—5	—7	3	—1	1	2	2	16	—1	—8	—8	–
—4	6	2	—1	—3	0	5	4	2	—8	—3	—4	—2	—11
—1	—2	—1	—4	2	—3	—8	—7	—8	6	—2	—12	—17	—19
—2	—2	0	0	1	0	1	0	2	4	—3	—7	—7	—7
—2	7	6	6	5	5	6	6	5	—6	—2	—5	—3	–
—1	3	—2	0	2	4	7	8	7	—3	0	2	2	–
0	1	0	0	—1	1	5	2	7	—1	—1	1	1	–
—1	—1	1	2	2	1	1	4	2	0	—2	2	3	–
—2	4	5	2	1	1	15	11	16	12	1	—7	—13	–
—5	—2	0	—6	—5	5	9	5	8	3	—6	—2	—2	–
—2	—1	—1	—1	3	—1	—2	—1	—1	3	0	—2	—4	—6
—2	2	—2	—1	4	—2	1	—1	1	2	—3	—1	—2	—4

portant, because the mean values here are more representative of individual trends than are those of the lower blocks. There was in every case of the 10 a significant increase in the *circumference of the upper arm,* both loose and contracted.

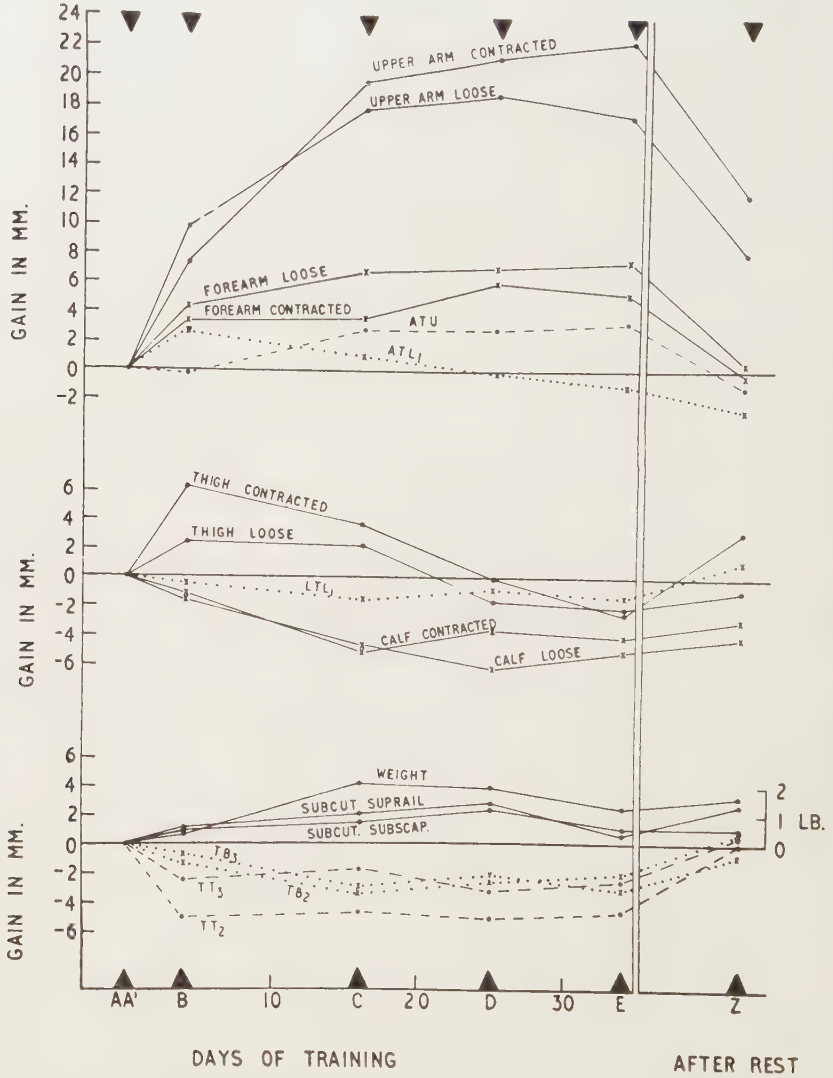

Fig. 1 Average changes in measurements for 8 individuals during and after 4 months of weight-training.

The gains ranged from about 10 to 30 mm, which is about 4% to 11% of the pre-experimental arm circumference. The greatest individual gain was 13%. During the 4 months post-training period all subjects regressed from their maximum training figure, two completely back to their pre-training level, the rest, with one exception, at least halfway back. These changes are due to muscular growth and recession; there were no concurrent changes in subcutaneous tissue over the front or back of the upper arm, and the muscular increase can be clearly seen in the photographs.

In the *forearm circumference* there was in all cases a gain, though it was of insignificant proportions in one individual. The gains were from about 3 to nearly 20 mm for both loose and contracted, which is 1% to 7% of the pre-experimental forearm circumference. Nearly all this increase in tissue is lost again, and at Z only one individual had a value significantly above pre-experimental.

In the middle block of figure 1, the average leg measurement changes are given. The two solid lines starting by rising above the control level are for *thigh circumference,* relaxed and contracted. These changes, anyway not so nearly so large as the arm ones, are less representative of any individual. Five subjects decreased significantly during the training period, three had no change, and two (subjects G and M of table 1) gained. Of those who decreased, two regained some of the lost tissue at the end of training, two did not change, and one was not seen. Both those who gained during the experiment lost most of their new tissue after it. It seems likely that all or nearly all of these changes are due to loss or gain of subcutaneous fat, and not muscle. In individuals they tend to run parallel to changes in subcutaneous tissue of back and abdomen; one of the most labile of the externally visible stores of fat seems to be the pocket just under the fold of the buttock. Two happenings between B and C in this experiment may have influenced fat stores; the Easter holiday of one to two weeks almost directly after B, and the increase in severity of training which began just a week before C.

The *calf circumference* changes are more consistent from one individual to the next. All decreased during training, from about 2 to 10 mm, which is from $\frac{1}{2}\%$ to nearly 3% of the pre-experimental value. There was a small return towards the pre-training value at the end of training, reaching significance only in three individuals. Presumably this decrease is due to loss of fat, at least in the relaxed calf. Admittedly the shape of the gastrocnemius is such that a decrease in maximum calf circumference takes place on contraction of the muscles, but there is little reason to suppose a similar alteration would take place in the relaxed part following muscular growth.

The *photogrammetric limb diameters* in general follow the same sort of curves as the corresponding circumferences, as can be seen for ATU (the upper arm diameter in the side view, triceps contracted) and ATL_1 (maximal forearm diameter in side view) in the upper block of figure 1. The apparent decrease rather than flattening out of ATL_1 after session B should not be taken too seriously; this is the least reliable of all photogrammetric measurements, and may be decreased by the subject learning to hyperextend at the elbow more fully at sessions subsequent to the first. The mean changes in LTU_1 (thigh diameter taken directly under fold of buttock in side view) were insignificant and they are not plotted; LTL_1 changes (maximal calf diameter, back view) can be seen in the middle block of figure 1.

The inclusion of these diameters taken at the same level as the corresponding limb circumference makes it possible to get some idea as to the *changes in shape of the limbs* that have occurred as the result of training. The data necessary for this are set out in table 2. Consider, for example, calf circumference, and the transverse calf diameter LTL_1. The regression of circumference on diameter in a group of 70 individuals was shown to be 3.03 (Tanner and Weiner, '49). This is practically π, and indicates that the calf cross-section is almost a circle: in going from a small man to a large man, the circumference on the average changes 3 units for every unit change of LTL_1 diameter. Now the regression *within* each of the 10 subjects of the present experiment is calculated. There are 4 or 5 pairs of values for each subject; they are couched as gains in circumference and gains in diameter, but the regression is the same whether for absolute values or gains, since changing from one to the other only involves a shift of origin. Each individual has a regression coefficient and these can be average for the 10 individuals. The average regression of calf circumference gain on LTL_1 gain is now found to be 0.67, a figure greatly (and significantly) different from 3.03. As a man loses one unit in calf diameter, he does not lose three in circumference, but only 0.67. In other words, he loses proportionately much less in anteroposterior diameter than in transverse diameter, the "proportionately" referring to the amount that would be lost in travelling the path from the average shape of a large man's calf to the average shape of a small man's. Relative to the between-individual shape change, the weight-trained man has lost more tissue transversely and changed the shape of his calf to a more oval cross-section.

TABLE 2

Comparison of regressions of limb circumferences on photogrammetric diameters to show shape changes under training

DIMENSIONS	REGRESSION COEFFICIENT		CORRELATION COEFFICIENT	
	Av. within individual 44 obs. on 10 subjects	Between individual 70 subjects [1]	Av. within individual 44 obs. on 10 subjects	Between individual 70 subjects [1]
Arm circ. on ATU	1.22	1.72	0.62	0.70
Forearm circum. on ATL_1	0.29	1.45 *	0.23	0.68
Thigh circum. on LTU_1	0.99	2.15 *	0.54	0.91
Calf circum. on LTL_1	0.67	3.03 *	0.35	0.93

* Signifies a significant ($P < 0.01$) difference between this figure and corresponding within-individual regression coefficient.

[1] Data from Tanner and Weiner ('49).

The first column in table 2 gives the average within-individual regressions, which are the average of the 10 individual regressions; they were actually calculated as the within-individual figures in an analysis of covariance on 44 pairs of observations from 10 individuals. The second column gives the between-individual regressions, taken from the data of Tanner and Weiner ('49) on 70 untrained subjects. The between-individual regressions that are starred differ significantly, at the 1% level, from the corresponding within-individual regressions. The between-individual correlation coefficients are also given, to enable the closeness of the relationships to be judged.

The within-individual regression of arm circumference on ATU is 1.22 and this does not differ significantly from the between-individual regression of 1.72. The change in cross-sectional shape of the upper arm occurring in a single individual under the stimulus of weight-training does not proceed along different lines to those changes existing between small and large individuals. Or, more accurately, our not very voluminous data do not show any reliable differences; what indication there is points to the training gain in circumference being less per unit increase in ATU than expected from the between-individual relation: in others words the ATU diameter developing proportionately more than the diameter at right angles to it. One would expect this from the location of the triceps and biceps, and from looking at the photographs.

The other three regression comparisons are all significant for circumference on photogrammetric diameter. Forearm circumference increases less in training per unit ATL_1 increase than between individuals; the forearm growth is proportionately greater in the ATL_1 diameter than in the diameter at right angles to it. The new muscle, as one would imagine, bulges out at the side of the forearm and increases the anteroposterior dimension relatively much less. The shape changes in upper and lower leg are in the same direction. Thigh circumference decreases less per unit decrease of LTU_1

than to be expected on the basis of the between-individual regression; most of the thigh shape change in training is situated along the LTU_1 axis. This supports the visual evidence that the change concerns chiefly the fat depots at the meeting of thigh and buttock, where the posterior end of the LTU_1 measurement is located. The major change in calf circumference is in the opposite plane, along the LTL_1 axis, across the back of the calf. Fat appears to come off the sides of the calf muscle, where as a rule there is probably more of it than in the front and back of the calf.

The lowest block of figure 1 shows the mean changes in *weight; subcutaneous tissue* under left scapula and above left iliac crest; TT_2 and TT_3 (two photogrammetric dimensions of anterposterior body thickness, at the minimum waist, and the level of the top of the pubic hair, respectively); and TB_2 and TB_3 (photogrammetric body breadths at waist and maximum hip width). The weight changes are given by the scale on the right-hand side of the figure; there was an average gain up to time C, and a very slight loss thereafter. Only 5 subjects fluctuated more than 1½ pounds; the changes for three of these are given in detail in table 1, the 4th lost 5 pounds between D and E and regained most of it by Z, the 5th gained 4 pounds steadily to D, lost three pounds, between D and E and regained it from E to Z. The subcutaneous tissue measurements show a slight overall gain during training, but this was chiefly due to subject M. Two other subjects did register significant gains, however, and there were no significant losses. The photogrammetric measurements of trunk thickness and trunk breadth, TT_2, TT_3, TB_2 and TB_3 show a decrease during training followed afterwards by an increase to the initial figure. The decrease is greatest for TT_2, the antero-posterior diameter of the waist, the average loss being about 5 mm which is 2½% of the pre-training value. Individual losses were significant for TT_2 in 8 cases, TB_2 in 7 and TT_3 and TB_3 in 5; other individuals' changes did not reach significance except that subject M gained significantly in TT_3. This decrease is therefore a fairly consistent one, and presumably represents a loss of subcutaneous fat over the front of the epigastrium chiefly, but also at the front of the hypogastrium and at the sides of the waist and hips. On the whole those subjects with most subcutaneous fat tended to lose most, but the tendency was not very marked and this data is insufficient to say whether it was a chance occurrence or not.

The loss of tissue as revealed by photogrammetric measurements bears little relation to the change in subscapular and suprailiac subcutaneous tissue. Not only did the means of the series go divergent ways, but several individuals lost in all 4 photogrammetric measurements at the same time as they gained in subcutaneous caliper measurements. This may mean that intraabdominal rather than subcutaneous fat was lost, or simply that the techniques used measure somewhat different things. The subcutaneous caliper measurement is made by picking up a fold of tissue with a wide pinch of thumb and forefinger, and is influenced by the character of the tissue, whether it easily pulls up from the underlying muscle, and whether it is easily compressible by the calipers (and, one may remark, by the tension of the caliper spring, which can vary considerably from one instrument to another; in this experiment, however, only one instrument was used throughout). The photogrammetric measurements measure changes in contour and one cannot necessarily ascribe these to fat changes, though inspection of serial photographs does reveal to a surprising degree whether or not this is mainly the case. They are also affected by postural change, though of course every precaution is taken to reduce this to the minimum.

The other photogrammetric dimensions, all of which are recorded in table 1, did not show any significant average change; nor did stature, biacromial diameter, chest circumference or expansion, or bicondylar diameters of humerus and femur.

Changes in three individuals

In table 1 are given the raw data on 3 of the 10 individuals. The main block of figures are gains in millimeters over the average of the two pre-training values; immediately before them are given the pre-training figures themselves.

Subject G, age 20, somatotype 263, was the person who attended most regularly, and more than any other subject; he worked harder than any other, and appeared keener. Mister Atkin reported of him: ''He hardly missed a work-out, and was invariably the first to arrive and the last to leave. He always did as he was told and used heavy poundages; a really athletic-minded youngster who could be relied upon to do his best in any branch of sport.'' As a good deal of our individual differences in result probably spring from the degree to which different subjects pushed themselves in the work, G might be expected to show at least as great effects of training as any other, and in muscular building this is indeed the case. He gained over an inch in contracted upper arm circumference, and half an inch in the forearm. Half of this gain had disappeared 4 months later, however. Between B and C he gained 6 pounds in weight but then declined from C to E. Exactly parallel changes took place in thigh circumference, and within the limits of error, in LTU_1 and in subscapular and suprailiac subcutaneous tissue. These changes plot a course directly opposite to the arm ones, and evidently represent fat deposition and loss. There is in addition a steady loss, presumably of fat, at TT_2 and TT_3, with a gain at the end of training. There is the usual diminution of calf circumference. The remaining changes are insignificant. It is perhaps worth mentioning that G had an examination at about time E, and this may have influenced the results for fatty tissue and weight.

Subject M, age 21, somatotype 2½53, represents one extreme of our 10 subjects; he gained weight steadily throughout his training period of 36 attendances and sustained the new level thereafter. Evidently most of this was due to fat deposition.

Whether this was the result of the training, by stimulating appetite or by other means, cannot be said; he may have been underweight to begin with, though this is not at all obvious in his initial photograph. That he kept his added fat after the end of training is perhaps evidence against his gain being due to the exercise. M increased his upper arm circumference even more than G, but this time some of the gain is fat; if we can trust the figures for subcutaneous tissue, the fat was laid down mainly at the back of the arm. The forearm circumference increased to about the same extent as G's, and it seems likely that this was mainly muscle, both because of the anatomical location and the loss of the extra tissue by time Z. There is a marked and fairly well sustained gain in thigh circumference, in LTU_1 and LTU_2, in subscapular and suprailiac subcutaneous tissue, and interestingly enough, in NTt, the breadth of the neck, as well. Despite the general bodily gain, the calf circumference decreased, and so did TB_2.

Subject C, age 18, somatotype 2½5½2, represents the opposite extreme. He lost 7 pounds during his 23 training attendances, and a glance at his photographs suffices to show that most of his loss came directly from the front of his abdomen. He was at first considerably overweight for his physique, and his final picture at E looked far more like the majority of young men of this somatotype. Unfortunately he did not attend after the end of training, so we do not know whether he put on weight once more or not. He put on some muscle in the upper arm and forearm, but gained nowhere else. The thigh circumference decreased by about an inch, LTU_1 decreased, and his calf circumference fell by as much as half an inch. His subscapular and suprailiac subcutaneous tissue decreased a little, and the anteroposterior waist diameter decreased by an inch and a half. The waist breadth TB_2 decreased almost as much, while TT_3 and TB_3, lower down, fell also, but somewhat less. None of these changes is due to a change in posture; the curves of the back in the side view are completely superposable in the pictures at A and E.

SOMATOTYPE CHANGES

The second question that concerns us, is what changes if any occur in the estimation of the somatotype (1) by anthroposcopy and/or (2) by Sheldon's anthropometric method. Whether one regards the somatotype itself as changing if the bodily measurements change is purely a matter of verbal definition; most workers in this field define the somatotype as the constitutionally fixed element of physique, entirely or largely gene-determined, or at any rate not modifiable when ordinary growth has ceased. Under weight-training, the muscles may bulge, but the somatotype remains. Whether one can always or usually recognise the man who has trained his muscles to above their ordinary size, as one can often recognise with practice even the mildly starved, remains to be seen. If one can, presumably allowance should be made in both visual and metric estimating procedures. With the present data, however, we are merely concerned to see whether the relatively mild degree of training throws the ordinary methods of estimation badly out or not.

Somatoscopic

To begin with, an attempt was made to place all the pictures of a particular individual in their right temporal sequence, the numbers still being obscured. The changes that occurred during training by no means leaped to the eye. In only 3 out of the 10 subjects could the pictures be placed in a practically correct order, but in 9 of them it proved possible to identify either one or both of the pre-training pictures, and in 7 the last or the next-but-last was placed at the end of the row.

The main changes were in the upper arm and shoulder girdle. In nearly every subject there had been sufficient growth of the biceps and triceps for the change to be visible, and in most a development of the pectoralis major, the trapezius, the teres major, the latissimus dorsi and the deltoids could be made out. In some the sternomastoid seemed a bit more prominent, and in three there seemed to be a slight loss of fat over most parts of the body, but this was not marked. In nearly every subject it was the biceps which had been most obviously developed. Away from the upper arm and shoulder girdle no changes could be seen, except that despite a rigorous posing technique the posture in most cases changed very slightly, the subject standing a little bit straighter after training.

In no instance was the change due to training sufficiently large to change the anthroposcopic estimate of somatotype on a 7-point scale. Possibly changes of one-half in some overall (i.e. total body) ratings might have been made, and perhaps, though doubtfully, a change of one in the regional rating for the arms in one or two subjects. This is, of course, on the assumption that the observer had no knowledge that any special training was being indulged in by the subjects. It is quite possible that the estimate of dysplasia would undergo some alteration — mainly in the direction of puzzling the observer by the peculiar growth of one region — but it would need more bodily change than that induced here before the overall estimate of the somatotype was seriously affected.

Somatometric

The anthropometric estimates of somatotype from the pictures at A and E bear out this conclusion in detail. These estimates were made by the purely objective technique, without using any somatoscopic typings at all. The subject's measurements for each region were matched against each one of the whole range of 76 somatotypes and the regional somatotype with the lowest average departure from the measurements in hand was recorded, Sheldon's tables being used. We have found certain difficulties in applying this method, which have been gone into elsewhere (Tanner, '51a); suffice it to say that from time to time an anthropometric regional estimate occurred which was obviously at variance with the physique and with other regional ratings. This may or may not be due to posing or age of the subjects; when it occurred, a somatoscopic regional estimate was substituted.

The overall ratings for the 10 subjects showed in no instance a change greater than one-half in any one component between A and E. The mainly-anthropometric estimates are given in table 3. Some, but not all, of these one-half rating changes are due to training; some would have occurred anyway due to small errors of measurement occurring from one time of posing to another, which may sometimes raise a regional somatotype by a rating and thus sometimes an overall somatotype by a half rating. It is noteworthy that in endomorphy there are two increases (of one-half rating) and three decreases; in ectomorphy one increase and two decreases; and in mesomorphy 4 increases and no decreases.

TABLE 3

Validity of mainly-anthropometric estimate of somatotype before and at end of training. See text for details

SUBJECT'S AGE	ESTIMATE OF SOMATOTYPE AT *A*	ESTIMATE OF SOMATOTYPE AT E	$HT/\sqrt[3]{WT}$ AT A	$HT/\sqrt[3]{WT}$ AT E
C. 18	35½2	2½5½2	12.6	12.8
19	44½2	452	12.8	12.7
G. 20	263	1½63	13.0	13.0
M. 21	253	2½5½3	13.2	13.0
21	25½2½	25½2½	12.9	12.9
21	245	2½45	13.5	13.5
22	44½2½	452	12.4	12.5
22	24½4	254½	13.3	13.3
25	3½4½4	34½4	12.9	13.0
25	2½5½3	2½5½2½	12.8	12.8

The effect of this training on the estimates of regional somatotypes and on dysplasia is more serious. The increased upper arm diameter with an increase in the forearm not sufficient to balance it produced in two cases an absurd anthropometric arm estimate after training; 452 became 631 and 262 became 622, the increased upper arm muscle being mistaken by the tables for the more usual hamming of the endomorph's fat. (This sort of error could not occur in visual estimates, for though the shape has changed towards that usually seen in endomorphs, it is perfectly obvious to the eye that the tissue is muscle.) The training changes are insufficient to bring this about in most cases, however; for the most part changes are only to a neighbouring somatotype.

STRENGTH CHANGES

Four tests of strength were made by Mr. Atkin on each subject before the beginning of the experiment at time D, and on 5 subjects at their last attendances. The tests were the maximum poundage that could be lifted on two-hands press, straight-arm pullover, two-hands curl and deep knee bend. Every subject increased in strength, by 5 to 20 pounds on the press, by 5 to 30 pounds on the pullover, by 5 to 35 pounds on the curl, and by 70 to 180 pounds on the deep knee bend. These increases represent an average gain of about 10%, 20%, 20% and 80% respectively of the initial poundage lifted. It should be added, however, that some of this ability to lift heavier poundages is due to the acquiring of technique rather than to increased strength of muscle; technique plays a particularly large part in the deep knee bend.

CREATININE EXCRETION

There were no significant changes in 24 hour creatinine excretion during the experiment. The most complete record was that of subject G; for times A, A^1, B, C, D and E the figures were 2045, 2016, 2187, 2010, 2100 and 2000 mg. Subject M had figures for A, A^1, D and E of 1834, 1740, 1800 and 1770 mg; subject C for A, A^1, C, D and E of 1900, 1840, 2020, 1850 and 1650 mg.

DISCUSSION

There seems to be very little serious literature dealing with the quantitative aspects of form change under training. Seltzer ('46) measured the chest circumference in mid-inspiration of 272 American Air Force cadets before and after an 8-week course of severe physical training of the usual type. There was an average increase of 0.60 cm with a standard deviation of approximately 2 cm. Almost as many lost as gained, in other words, and some losses and gains were over 5 cm. There were 37 men who lost over 1.50 cm, and of these 35 lost weight concurrently, their average loss being no less than 9.6 pounds. In contradistinction to this, there were 61 who gained over 2 cm; and of these only 51% lost weight, and 36% gained weight. Whether there was a gain or loss seemed independent of initial chest size, but these data make it clear that it depended heavily on physique. Obviously it was fat that was lost, and probably it was mainly muscle that was gained.

A more extensive series of measurements was followed by Jokl, Cluver, Goedvolk and de Jongh ('41) in 32 young South African unemployed men who underwent 24 weeks of army-type physical training in a Special Service Battalion under para-military discipline and auspices. Measurements of stature, sitting height, biacromial, bicristal, bitrochanteric, interspinous, transverse chest and anteroposterior chest diameters and chest circumference were taken at the beginning of training and at 6 week intervals during it. The authors most commendably publish all their raw figures for each individual so that it is possible to divorce their results from their conclusions. It must be emphasised that half their subjects were under 18½ years old and therefore some of them certainly not at the end of their normal growing period; also they were probably most of them undernourished on arrival. All except two gained weight, the largest gain being about 19 pounds, and a more usual figure about 8 pounds. Despite this, however, increases in the linear dimensions were conspicuous by their absence. It is hard to know just how large a change in the individual figures may be due to chance, since the method of measurement used is not given, and the diameter figures are mostly recorded to the nearest centimetre only (with occasional halves and quarters), implying the use presumably of some instrument other than the usual and accurate anthropometer. Stature and sitting height increased in about half the subjects, but, as the authors remark, this was almost certainly due to the initial measurements being taken when the subject's posture made the correct measurement hard to obtain. Adopting as near as possible the same criteria as in the present study, it would seem that there are one or two unequivocal increases in biacromial diameter, probably due to postural change, none in chest transverse or antero-posterior, bicristal or interspinous diameters (the last with the exception of one possible gain and one possible loss). The authors consider bitrochanteric diameter to have increased (by an average figure of 8 mm) but the data seem to show only two certain cases of this, and 5 doubtfuls, the latter having only sudden increases at quite late stages of training. Only the excursion of the chest showed considerable change in a fairly high percentage of the subjects. We must conclude that this training produced no appreciable change in these measurements, but as they are all skeletal dimensions, this is not surprising.

The older literature can best be approached by way of Arnold's ('30) long paper, which is mainly a review of previous work, and has an extensive bibliography. Arnold is highly partisan in his conclusions but not so in the presentation of his evidence. He himself studied the effect of the regular course in gymnastics for students at the Leipzig Hochschule. These students were self-selected and mostly high in mesomorphy. The course included track and field athletics, swimming, boxing, gymnastics, rowing and wrestling, and occupied an average of between 6 and 8 hours a week during term-time. There were 145 students followed for 6 semesters, that is three academic years, and 202 students followed for three semesters. The average age of the students was 20.7

years, and therein lies the objection to Arnold's results. The range of ages is not given, but the stature of the first group increased 4 mm over the 6 semesters and of the second group 4 mm over the three semesters. It seems that some ordinary growth was still proceeding, since it is unlikely the exercise stimulated growth in stature, and since evidently some of the students must have been 17 and 18 to produce the mean figure of 20.7 years.

The increases recorded in other measurements have to be viewed with this evidence in mind. For the first group (the second follows it closely) the gains over 6 semesters are: weight 4½ pounds, shoulder breadth 4 mm, hip breadth 2 mm, chest breadth 3 mm, chest depth 0 mm, neck circumference 8 mm, chest circumference 32 mm, waist circumference 3 mm, upper arm circumference extended 8 mm, flexed 8 mm, forearm circumference 4 mm, thigh circumference 4 mm, calf circumference 3 mm. Just how much of these gains are due to exercise and how much to the normal growth of nearly two years, it is very hard to say. It seems likely there has been some extra increase in neck and upper arm as well as chest circumference, but the arm gain is a good deal less than in the present study.

Kohlrausch ('25) reported an experiment on 67 subjects which is not open to this objection. Nearly all were over 20, and the average age was 23. They were students at the Deutsche Hochschule für Leibesübungen and were subjected to two semesters of the same sort of exercise as Arnold's group only much more intensively. They trained for no less than 4 hours daily during term-time. There was no significant change in stature during the year. The increases in other measurements were: weight one and one-half pounds, shoulder breadth 2 mm, pelvic breadth (bicristal) 3 mm, hip breadth (bitrochanteric) 24 mm, chest breadth — 2 mm, chest depth 0 mm, chest circumference 42 mm, waist circumference 0 mm, upper arm circumference extended 7 mm, flexed 6 mm, forearm circumference — 1 mm, thigh circumference 2 mm, calf circumference 11 mm. The last seems rather a surprising figure, but the others agree adequately with such other data as we have. The increase in hip breadth is presumably due to growth of upper thigh muscles.

Kohlrausch most interestingly remarks that those students who at the beginning of the course were thinnest in muscular (e.g. limb) diameters gained least, and those who were thickest gained most. The parable seems to extend even to the confines of body-building; those Kohlrausch classified as slender, in fact, made no significant gains at all except in chest circumference.

There are lastly two rather smaller studies by Herxheimer ('21, '22). The first concerns the contrasted growth of 10 young men who came seldom to evening athletic practice throughout a summer with that of 29 who practised keenly. The ages were 16 to 20, so considerable growth in all dimensions was still to be expected. Thigh circumference grew 70 mm in the lazy, but 143 mm in the keen. Whether this difference is significant cannot be judged and in any case the two groups were of considerably different build when exercise began and so probably at different places on their average growth curves. The second study was done on adults aged 26 or thereabouts. A total of 51 subjects exercised strenuously for 3 to 4 hours a day for 10 weeks, and as a result gained an average of 4 mm in thigh circumference and 4 mm in upper arm circumference: the gains at 5 weeks appeared to be slightly above those at 10, but probably not significantly so.

None of these previous studies gives any test of the significance of the increases described, and none includes a report on the subjects after they have passed a little time without the training or exercise.

As to the effect of exercise on the growing child, the literature here is as vast as that on the adult is scanty, and the conclusions if anything still less certain. Perhaps a not unfair summary of the situation may be about as follows: (a) There is little evidence that growth in height or limb length (or probably any skeletal dimension) is stimulated by exercise: some investigators have alleged it to be retarded but the evidence for this is very poor. (b) Growth in circumference of chest and limbs may be stimulated, though there seems to be no cast-iron evidence of this; that is, evidence which does not involve the element of selection of subjects, for properly paired controls are hard to come by. However it seems reasonable to suppose on the basis of the adult data that such an effect may occur. (c) If so, we do not know for certain whether this is a hypertrophy of muscle fibers such as occurs in the adult or whether the laying down of new fibers is stimulated. It is thought, however, that few if any new fibers appear in the human after birth, the enlargement of the muscle mass being purely a matter of increase of fiber size (White House Conference, '33). There is evidence that this also is the case in the sheep (Hammond, '32). If this is so, it seems quite probable that the fibers stimulated by exercise in the child would revert to their normal size for the child's age after the exercise had ceased; there would thus be a period of slower overall circumference growth after cessation of exercise. There seems to be no good evidence as to whether in fact this occurs.

Even the literature on experimental animals is a little more contradictory than one would like, though it seems clear enough that hypertrophy of muscle fibers in some muscle groups in some animals can be brought about by exercise provided the exercise is really severe (Steinhaus, '33). Lippold ('49) has recently demonstrated increases in fiber cross-section of about 15% in the gastrocnemii of rats following electrical stimulation, but could obtain no hypertrophy at all by exercising the animals on a treadmill, even for prolonged periods. The reverse effect, of complete rest causing a diminution of muscular mass in man, seems well established (Deitrich, '48).

With regard to the results of the present experiment, a few points seem worth making. The technique, a combination of photogrammetry and direct measurement, but with considerable emphasis on the former, seems quite well suited for the study of small changes in physique and could perhaps be used to advantage in the study of the effects of endocrine preparations, nitrogen loss or gain, changes in pregnancy and so forth in the human, where it might serve to point accurately *where* weight was gained or lost, and suggest the nature of the tissue changes at the various sites.

Stereo-pair pictures — the full aerial survey technique instead of half of it — would do the job still better, but take about 10 times as long to measure and require a highly expensive measuring apparatus possessed only by a few specialised institutions. Roentgenograms differentiating bone, muscle and fat such as are used routinely in many growth studies would be advantageous. Even with the present technique, however, the analysis of the weight gain and loss of subjects M and C respectively (table 1) could be carried quite far.

Secondly, it must be clearly understood that this experiment is a very limited one. It would be rash to predict from the present investigation the effects, temporary or permanent, of long-continued chronic exercise of particular muscle groups as in some sorts of manual labour or some athletic tasks. The effect during the time when the subject is still growing cannot be foretold from these data; neither can it be said whether our subjects would gain more rapidly when trained a second time, as some instructors allege. Against this, we can argue that this type of weight-training is likely to increase muscular bulk if anything does, since it is specifically designed to that end; that the shape of the body can to some extent be altered is the body-builder's whole claim and title to professional existence. It should however be said that the instructor did not regard these 10 subjects as on the whole particularly hard-working; they may give a fair picture of the effect of weight-training on the average mesomorphic young man, but really keen lifters and body-builders are alleged to produce considerably greater increases in muscle size.

The opinion of body-building instructors is also on the whole in agreement with the results of Kohlrausch's experiment, in holding that men high in ectomorphy, or linearity, are harder to enlarge than those already very muscular. Whether there is under ordinary circumstances a difference in average muscle fiber cross-section between these two groups is not a settled question, but such has been alleged by Wiersma ('33), and appears in some of the older literature (Halban, 1894, as cited in Hammond, '32). Nothing is known about habitual differences in chemical content or function.

Lastly, the change in *form* of these subjects is of considerable interest. The arm muscles grew considerably; the leg muscles, as far as can be judged, not at all. The training was not designed to produce this result, but on the contrary a general increase in muscle all over the body. Nor is this an isolated finding; I am told that the legs, and in particular the calves, are always more difficult to make grow than the arms. They need more exercise, and even then respond less. Agreeing with this is the fact that runners do not develop obviously increased calves or thighs, though these are the main parts they exercise. Indeed runners often have conspicuously slender legs. It would be very interesting to study the changes in thigh and calf formation during training in racing cyclists and ballet dancers.

This apparently different distribution of growth potential in arms and legs has no very obvious explanation at the moment. It may be said that the differential growth effect is not due to any inherent difference in muscle responsiveness, but to a relative failure, despite appearances and protests, to exercise the legs in training. Such a failure, if it exists, might be partly due to the undoubted unpopularity of deep knee bends and calf exercises amongst body-builders and lifters, and partly to the technical difficulty of devising a highly efficient calf exercise, because full extensions and flexions of the calf muscles apparently cannot be performed with near maximum poundages. It may be that the shape of the muscles and not their chemistry prevents them becoming very large and powerful.

On the other hand these explanations are by no means entirely convincing: nor, really, is the argument that we use our legs more than our arms and therefore the former are nearer the maximum size ordinarily. In rats different groups of muscles respond differently to the administration of growth hormone, some growing a lot, others a little (Greenbaum and Young, '50). The same experiment provides a concrete example of growth in bulk of muscle occuring without increase of strength (Bigland and Jehring, '52). Muscle fiber size has been very inadequately studied in man, but it is clear from Hammond's work ('32) that in the sheep considerable differences in average fiber size exist between one muscle and another; and the older study of Mayeda (1891) showed the same to be true of many and various species. It is said that in man, mouse, the frog and the bird the fibers of the gastrocnemius have a larger cross-section than those of most or all other skeletal muscles; in the sheep they are about the average size for all the muscles of the leg. Comparisons between species, however, are largely vitiated by differences in habitual function and the subject needs reinvestigation, particularly in man. That some difference in structure exists between leg and arm muscles is indicated by the finding of Rexed ('44) that the ventral spinal nerve roots in man contain in the cervical region (C7) some 10–25% of small-motor-nerve fibers while the roots of the lumbo-sacral region (S1) contain less than 5% of these. But there is no reliable data on the muscle fiber size or muscle enzyme content of the human gastrocnemius compared with the biceps or triceps; perhaps the difference of growth potential would provide a lever in the analysis of the physiological mechanisms of muscle growth.

SUMMARY

1. Ten healthy young men of predominantly mesomorphic physique have undergone a course of physical training by

weight-lifting over a period of approximately 4 months. The effect of this on their physique has been followed by taking some 20 anthropometric measurements twice before the training began, every three weeks during training, and 4 months after training had ceased. At each measuring session photogrammetric photographs were taken and 19 more measurements were obtained from these.

2. There were considerable individual differences in response, and the original raw data for three individuals are presented in table 1; the average changes which reached statistical significance are plotted in figure 1.

3. The largest increase was in upper arm circumference, in which every subject significantly increased. The average gain was approximately 2 cm; the maximum nearly 3½ cm. The forearm circumference also increased, but the calf circumference decreased in all subjects. Subcutaneous tissue measurements remained unaltered, or increased, but the photogrammetric trunk measurements mostly decreased.

4. Four months after cessation of training all measurements tend to have reverted to their pre-training values. Most approximately reached them, but upper arm circumference still averaged 1 cm above the initial figure.

5. The experiment was conducted partly as a test of the technique of photogrammetric anthropometry and, in conjunction with living body measurements, this appears to have worked satisfactorily. The technique allows the changes in shape of the limbs to be recorded, and these are discussed.

6. The extent of the change produced would not lead to an erroneous anthroposcopic somatotype being given to the subject by a reasonably skilled rater, even if the fact of this unusual degree of exercise were unknown; it might however affect the estimate of dysplasia. The effect on the use of Sheldon's anthropometric somatotyping tables is discussed.

7. An interpretation of the anthropometric findings in terms of fat lost and muscle gained is given, and the point made that the muscular growth potential of the arm muscles in the human seems to be considerably greater then that

of the leg muscle, particularly the calf. This is further discussed.

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NUTRITION AND DENTAL CARIES.— Carbohydrates have been blamed most often for the initiation of the carious process as substances providing an easily fermentable substrate which leads, through bacterial activity, to formation of acids on the surface of the teeth with subsequent decalcification of the enamel. Diets high in carbohydrates have been considered predisposing to caries. If this be true, the Oriental diet, supplying as much as 82% of the calories from carbohydrates, predominantly rice which gives an acid residue, should predispose to rampant caries. In fact, it has been stated that Polynesians eating taro (alkaline residue) have only 4.59% of persons affected by caries while Polynesians eating rice have an incidence of caries 98.5%, the inference being that the Oriental diet, rich in carbohydrates (rice), was responsible for the high caries rate. Our observations carried out among the Chinese consuming this type of diet disprove the validity of this statement. On the contrary, it seems beneficial in the production of caries immunity. Insofar as it has been demonstrated that the restriction of sugar either refined or natural is effective in the control of dental caries, we are prone to agree with the observers who assert that it is not the amount of carbohydrates consumed, but their type (mono- or disaccharides) and the degree of their refinement that play the important role. Our observations on children support this assumption. D. Afonsky. Some observations on dental caries in central China. J. Dent. Res., vol. 30, no. 1, 1951, pp. 52–61.

Human Hemoglobin.— Investigations in our laboratories on human hemoglobin during the past 4 years have shown that the erythrocytes of an individual may contain two, and even three, different molecular species of hemoglobin. Ten genetically distinct conditions that may be characterized by the hemoglobin composition of the erythrocytes have been observed. Some of these conditions are associated with severe anemias, but others are difficult, if not impossible, to detect on the basis of clinical observations. . . .

The discovery of an electrophoretically abnormal hemoglobin in sickle cell disease in 1949 (Pauling, Itano, Singer and Wells) provided the first positive evidence that adult hemoglobin may exist in more than one molecular form. The following year a second abnormal adult hemoglobin was discovered, again by the use of electrophoretic mobility measurements. A third abnormal adult hemoglobin was detected last year by the combined use of electrophoretic mobility and solubility measurements. . . .

The available evidence indicates that an individual inherits a genetically controlled mechanism for the synthesis of hemoglobin from each of his parents. The great majority of individuals have inherited a normal mechanism from each parent, and their erythrocytes contain only normal adult hemoglobin except in the first few months of postnatal life, when fetal hemoglobin is also present. Whenever an abnormal adult hemoglobin is present in an individual, investigation of his parents has demonstrated the presence of the same abnormal form in one or both of them. Matings of two individuals who possess different abnormal adult hemoglobin may result in children who have either or both the abnormal forms. . . .

Among individuals who have the sickle cell trait — i.e., those whose erythrocytes contain both normal adult and sickle cell hemoglobins — a wide variation in the ratio of concentrations of the two forms has been observed. The results of familial studies of this ratio are consistent with a hypothesis that different normal alleles may exist in the human population which control the production of normal hemoglobin at distinctive rates.

Simple, rapid tests for the detection of the different normal and abnormal hemoglobins and their mixtures are needed, both for chemical and genetic studies. . . . Harvey A. Itano. Human Hemoglobin. Science, vol. 116, pp. 3–4 (editorial), August 8, 1952.

ESTIMATION OF STATURE FROM LONG BONES OF AMERICAN WHITES AND NEGROES[1,2]

MILDRED TROTTER AND GOLDINE C. GLESER
Department of Anatomy, Washington University, St. Louis

FOUR FIGURES

The estimation of stature from length of long bones of the free limbs is often an important contribution to the identification of unknown human remains. The need for identification was realized, perhaps more keenly than ever before in the history of mankind, during the United States Repatriation Program. This program was established by an Act of Congress in 1944. It included the identification (when possible) of unknown war casualties and was assigned to the American Graves Registration Service under Quartermaster Corps. Identification Laboratories were established in suitable parts of the world and the aid of physical anthropologists was enlisted.

Interest in stature estimation from long bones is not new but the number of actual investigations on the subject is relatively few. The most significant report in the last century was that by Rollet in 1888. He measured stature and lengths of the long bones of 50 male and 50 female French cadavers ranging in age from 24 to 99 years and presented all pertinent data including not only the methods of measurement but also the individual measurements and the resultant tables for stature estimation. Stature measurements were taken "generally in the week which followed death" with the cadaver lying on a graduated stretcher. The soft parts were then dissected away

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from the long bones which were measured on the osteometric board of Broca in the ''fresh state'' without having gone through maceration. A ''certain number of the bones'' were remeasured 8 or 10 months later in the ''dry state'' and it was determined that they had lost in general 2 mm of their length. Thus, when stature is to be estimated from the length of ''dry'' bones it has been the practice to add 2 mm to the measured length of each bone before application of Rollet's tables. The greatest length of the humerus, radius, ulna and fibula; both greatest and bicondylar lengths of the femur; and the distance from the two condyles of the head (with the intercondyloid eminence in the opening of the board) to the extremity of the medial malleolus of the tibia were taken. The tables present the average length of each of the 6 long limb bones of each side of the body for a given range of stature.

The raw data of Rollet served as a basis for application of different methods by Manouvrier (1892 and 1893) and by Pearson (1899). Manouvrier excluded those subjects of 60 years of age and over, 26 males and 25 females. He stated that due to the effect of ''old age'' on the length of the trunk they had lost 3 cm of their maximum stature. From data on the remaining 49 subjects (24 males and 25 females) he derived tables of average stature corresponding to given long bone lengths. In other words, Manouvrier determined the average stature of those individuals who presented the same lengths for a given long bone, whereas Rollet determined the average length of a given long bone from those who presented the same stature. The values obtained by these two methods are not interchangeable. Manouvrier also indicated that 2 cm should be subtracted from statures obtained by means of his tables in estimating stature of the living.

Pearson applied stature regression formulae utilizing all of Rollet's cases, but limiting long bone lengths to those of the right side unless the right bone was missing in which cases he used the left. He was aware of the wide age range but included all in calculating the constants noting that 50 cases are hardly sufficient for this method of treating data. He also reasoned that, since there were as many old individuals with a stature above as below the median stature, "whatever shrinkage may be due to old age it is not of a very marked character in these data or largely disappears when a body is measured after death on a flat table." The mean stature of the 26 males over 59 years of age was only 1.77 cm less than the mean stature of the 24 males under 60 years of age; the older group of females presented a mean stature of only .04 cm less than the younger group. It has been noted elsewhere (Trotter and Gleser, '51a) that Pearson failed to take cognizance of the greater long bone length in the older group of females than in the younger group and that the older group, therefore, had been taller individuals in their younger years than the stature measurements after death indicated. Pearson made a most valuable contribution to the problem of reconstruction of stature but emphasized that his formulae and curves must not be taken as final, that they merely represent the most probable conclusions which could be drawn from the data at his disposal. He hoped for a wider range of facts, more refined analysis, experiment and observation. In the course of his discussion he stated that "the extension of the stature regression formulae from one local race — say, modern French — to other races — say palaeolithic man — must be made with very great caution" and "stature is quite as marked a racial character as cephalic index."

In 1929 Stevenson accumulated data on a contemporary group of 48 Northern Chinese male cadavers (no ages given) according to methods which were the same as those applied by Rollet. He calculated stature regression formulae which he believed were comparable in all respects to Pearson's formulae for the French and then applied the formulae of each race to the other. The result was a failure of the formulae of one race to give satisfactory prediction results for the second. He emphasized the need of additional data in the form of similar series of regression formulae based on comparable data from other races. Pearson was the editor of the journal in which Stevenson's report was published and thus had the opportunity to add a note to Stevenson's paper. He suggested that there should be some hesitation in accepting all the conclusions but stated frankly that he was prepared to admit that better results from regression formulae will be obtained by applying a formula peculiar to a race itself than by applying a formula arising from a second race.

Breitinger ('37) approached the problem with the statistical methods introduced by Pearson but his data were from living subjects. He pointed out that cadaver material is ill-suited since it mostly represents a certain selection of the population according to age, socio-economic status and geographical distribution; and that stature measurements of cadavers are encumbered with greater errors than stature measurements of the living. His subjects comprised 2400 German males of which 1400 were participants in an athletic meet in Munich in 1923 and 1000 were students in 1925–26. The average age was reported to be about 26 years. Measurements of pertinent divisions of the limbs were taken between certain bony prominences and thus were not as accurate as measurements derived directly from the bones themselves.

Telkkä ('50) presented a chronological review of the literature in addition to his own results based on 154 Finnish cadavers, 115 males and 39 females. The average age of the males was 42.3 years and of the females 50.4 years. The stature was measured on the "prostrate" corpse and the bones were measured after maceration and drying. The skeletons had not all been preserved intact and thus the number of bones of a kind available for measurement was somewhat smaller than the number of subjects. The statistical treatment comprised correlation and regression coefficients between stature and bone measurements.

The United States' program for the return from foreign territory of the remains of World War II deceased made possible the measurements of long limb bones of American military personnel. Such measurements on individuals whose identity had never been lost will contribute to the improvement of identification criteria and thereby help in establishing the identity of unknown remains. The records taken at the time of induction provided the stature measurements. Thus, for the purpose of evolving formulae for stature estimates of American White and Negro males from long bone lengths the desired combination of records was for the first time procurable, viz., the stature measured during life and bones at hand for measurement after death. In addition to the advantage of having living stature measurements of the same individuals whose long bones are measured, it should be noted that these subjects embrace not only a younger age span than cadaver series are known to do, but also a broader and more representative cross-section of the American population. Furthermore, it has been suggested repeatedly that formulae are most accurate when derived from an extensive number of subjects and are applied most suitably to the population from which they were derived. This report will present formulae for estimation of stature from long bone lengths based on American White and Negro military personnel.

In 1948 Stewart wrote "Someone should work up the extensive records of cadaver stature and bone lengths assembled at Western Reserve University and Washington University." Dupertuis and Hadden ('51) at Western Reserve University have responded by their analysis of "groups of 100 male and 100 female American Whites and an equal number of both sexes of Negroes from the Todd Osteological Collection." Stature measurements of these cadavers had been taken by Todd and Lindala ('28). The subjects were secured in the upright position by means which insured that the heels were fairly planted on the floor. Dupertuis and Hadden considered this stature measurement of cadavers to be equivalent to living stature. Their calculations of the regression formulae were based on the values of the bones of the right side only.

In further answer to Stewart the present report includes a study of evidence available in the Terry Anatomical Collection. This material has already served in a study of the effects of ageing on stature (Trotter and Gleser, '51a), a parameter which has not been considered, heretofore, in relation to stature estimation. By making appropriate allowances for the difference in age, it has been ascertained that these data for males and those from the military personnel yield essentially the same formulae. In addition, the Terry Collection supplies data from which equations for estimation of stature from length of long bones can be determined for the females of both races.

MATERIAL

The military personnel were drawn from American World War II casualties in the Pacific zone. The remains were brought by the American Graves Registration Service to Hawaii for preparation for final burial at which time the long bones were measured. Their remains had been skeletonized by natural processes during the temporary burials and the bones were clean and dry. All studied were American citizens of the male sex who had been born in the United States. Stature measurements had been recorded at the time of induction into military service.

The Terry Skeletal Collection is composed of complete skeletons of American White and Negro cadavers which had been assigned to the medical school for scientific study. The collection is well documented with respect to race, sex and age. Its constitution is similar to that of the Todd Skeletal Collection insofar as racial admixture of Whites and Negroes is concerned — even though there is not complete agreement on the question of extent of hybridization of the American Negro (Herskovits, '28; Terry, '29, '32; Todd and Lindala, '28).

The distribution of subjects contributing to this study from both military personnel and the Terry Collection is shown in table 1 according to source, race, sex and age. The age recorded for the military personnel is that at the time of induction into service when stature was measured; age for the Terry Collection is that at the time of death.

The right and left long bones of both upper and lower free limbs were considered, viz., humerus, radius, ulna, femur, tibia and fibula. When all twelve were present the list is referred to as complete; when one or more was absent, as incomplete.

The subjects of the Terry Collection were all complete and of the military personnel, 568 White males and 55 Negro males were complete. The incomplete group was classified under two categories: (a) absence of ulna(e) and/or fibula(e); and (b) miscellaneous absences.

The ages of the great majority of military personnel are in the late teens and early twenties. During this period the amount of increase in stature is small; and soon thereafter the maximum stature is reached. In order to utilize the data most effectively it was necessary to determine at how young an age stature does not differ significantly from the maximum stature. A decision to include all subjects of 18 years and over was based on findings which indicate that the amount of increase in stature after 18 years is insignificant. Randall ('49) in a study of age changes in 17,341 Army males, ranging in age from 17 to 26 wrote on the subject of stature,

TABLE 1

Distribution of subjects according to source, race, sex and age

AGE	MILITARY PERSONNEL		TERRY COLLECTION			
	White Male	Negro Male	White		Negro	
			Male	Female	Male	Female
17	46	3				
18	210	9				
19	105	7				2
20–29	676	52	1	1	46	31
30–39	76	14	11	8	66	38
40–49	2		37	3	69	36
50–59			53	8	76	26
60–69			86	16	65	16
70–79			52	18	29	19
80–89			15	9	9	8
90–99						1
Total	1,115	85	255	63	360	177

> "Even though the mean values indicate a maximum attained at age 24, there is no statistically significant change after age 18. Consequently, evidence is strong that the American White male attains his adult stature, as an average, in the 18th year."

The military series under present consideration support Randall's evidence. The mean statures according to race and age are presented in table 2. Actually, in the White group the average stature of the 17 year olds is greater than that of the 18 and 19 year old subjects, no one of which shows a statistically significant difference from the average stature of the total group. In the Negro group the number of subjects in these age categories is too small to justify conclusions. For both races, only those subjects who were 18 years and over at the time stature was measured are included in the statistical analysis. The subjects who were excluded on account of their youth consist of 46 White males (23 complete, 10 with absence of ulna(e) and/or fibula(e), and 13 with miscellaneous absences) and

TABLE 2

Mean stature (cm) and length of period (years) between stature measurement and death of military personnel according to age

AGE	WHITE MALES			AGE	NEGRO MALES		
	No.	Stature	Period		No.	Stature	Period
17 [1]	46	174.85	1.95	17	3	169.00	2.58
18	210	174.05	1.96	18	9	174.00	2.30
19	105	174.40	2.30	19–20	12	171.50	1.66
20	121	175.43	2.32				
21	94	175.14	2.47	21–22	21	172.29	2.84
22	95	174.32	2.41				
23	67	174.43	2.22	23–28	22	171.27	2.47
24	71	173.14	2.44				
25	73	174.15	1.81				
26	67	173.45	2.29				
27	31	172.52	2.50				
28	31	174.74	2.28				
29	26	173.27	2.49	29–37	18	173.06	2.36
30	26	173.23	2.61				
31–33	31	171.03	1.94				
34–48	21	172.27	2.82				
Total	1,115	174.23	2.25	Total	85	172.14	2.41

[1] A given year of age indicates a period from one birthday until the next. Periods of more than one year were made arbitrarily in instances where the frequency was small.

three Negro males (one complete, two with miscellaneous absences).

The average length of the period which elapsed between the measurements of stature and death is also summarized in table 2. It may be seen that the lapse of time is relatively short — slightly more than two years, on the average. It is only before maximum stature is reached that a disparity between times of measurement of stature and of long bones could introduce an error.

The Terry Collection subjects are all over 18 years of age but the range extends into the tenth decade and thereby introduces the need of correction for loss of stature with age increment after maturity. The correction formula is available (Trotter and Gleser, '51a) and thus stature measurements derived from these older subjects can be made comparable to those of the military personnel.

METHOD

The stature measurements of the military personnel were made under the direction of either the War Department or the Navy, thereby involving not only many different stations but many different observers. It is desirable to have all measurements of a given variable made by the same individual in order to keep the observational error at a minimum. Errors incurred by many different observers tend to reduce the correlation between variables but this effect is relatively small when sufficiently large series of observations are obtained. All observations were recorded in inches and have been transformed to the nearest centimeter. Numerous attempts have been made to learn the directions for taking height with the following results: in Mobilization Regulations, War Department, October 15, 1942, there was found:

> "10. Directions for taking height. Use a board at least 2 inches wide by 80 inches long, placed vertically, and carefully graduated to $\frac{1}{4}$ inch between 58 inches from the floor and the top end. Obtain the height by placing vertically, in firm contact with the top of the head, against the measuring rod an accurately square board of about 6 by 6 by 2

> inches, best permanently attached to graduated board by a long cord. The individual should stand erect with back to the graduated board, eyes straight to the front."

In another set of Mobilization Regulations dated 19 April 1944 essentially the same directions were given and in addition the following sentence:
"The shoes should be removed when the height is taken." An extract from Manual of the Medical Department, Revised 1945, United States Navy indicates:
"A minimum height of 60 inches without shoes is required." It has been assumed, therefore, that the statures listed on the records for all military personnel were taken with the subject in the erect position and with shoes removed. The stature acceptable for induction varied slightly among the service divisions with the extremes from 60 inches to 78 inches (152 cm–198 cm) inclusive.

The stature measurements of the Terry Collection subjects were made when the cadavers were brought to the Medical School. A specially constructed vertical measuring panel with a foot board was utilized.

> "With careful attention to the several details involved in posing and fixing the cadaver on the panel, the characteristic features of the standing posture can be reproduced: ankles bent, knees and hips extended, lumbar curve produced, shoulders squared and arms hanging at the sides, the face front and eye-ear plane horizontal." (Terry, '40.)

A metric scale was attached to the measuring panel. Each subject was photographed in anterior and lateral views. The photographs record the actual position of the cadaver and make feasible a correction in the stature measurement when, for example, the heels are not flat on the baseboard.

Measurements of length of all 12 long limb bones for Terry Collection subjects, and of as many as were present for military personnel were made by the senior author as follows and recorded to the nearest millimeter:

Humerus. Maximum length. Head was applied to the vertical part of the osteometric board, bone was held by left hand, block was applied horizontally to distal extremity, bone was raised slightly, moved up and down as well as from side to side until maximum length was determined (Hrdlička, '47).

Radius. Maximum length. Taken in same way as that of humerus (Hrdlička).

Ulna. Maximum length. Taken in same way as that of humerus (Hrdlička).

Femur. Bicondylar length. Both condyles were adjusted to the vertical part of the osteometric board and with the bone reposing on the board, the block was applied to the other extremity (Hrdlička).

Maximum length (indicated subsequently as femur_m). Medial condyle was applied to the vertical part of the osteometric board and measurement was made in the same way as the maximum length of other bones (Martin, '28).

Tibia. Maximum length (indicated subsequently as tibia_m). End of malleolus against vertical wall of the osteometric board, bone resting on its dorsal surface with its long axis parallel with the long axis of the board, block applied to the most prominent part of lateral half of lateral condyle.

Ordinary length. Measured with spreading calipers from the center of the articular surface of the lateral condyle to the center of the inferior articular surface (Krogman, '48).

Fibula. Maximum length. Taken in same way as that of humerus (Hrdlička).

The statistical analyses do not involve new methods. Regression equations introduced into this field by Pearson in 1899 and based on a linear relationship between the variables are proved again to be satisfactory. However, three refinements have been introduced: one, the utilization of stature measured on the living in combination with bone lengths measured after death on the dry skeleton; two, recognition of and adjustment for the effect of ageing on stature; and, three, a test of the validity of the resultant equations by application to a different sample of reasonably large size.

RESULTS AND DISCUSSION

Comparison of lengths of right and left bones. The 545 military White subjects who were 18 years or older and with all long bones present provided the data for comparisons between lengths of right and left bones. The object was to determine any possible difference resulting from utilization of one or the other bone of a given pair in estimation of stature. The complete matrix of intercorrelations is summarized in table 3. There is neither a large nor consistent difference in the amount of correlation for right and left bones of any pair except for the radius in which instance the left bone has a higher correlation with all other bones and with stature than does the right radius. Since the difference in standard deviation for any two corresponding bones is likewise very small there could be very little difference between estimation equations for stature evolved from them. There is, in general, a slight advantage in using the average length of the two bones of a pair when both are present, because of the greater reliability of an average. In addition, equations of estimation based on average values minimize the error of estimate when only one bone of a pair is present, since neither the right nor left member of a pair has a greater likelihood of preservation. Accordingly, it was decided to use the average length of bone pairs in this study.

The mean difference between right minus left bone lengths of a given pair and the standard deviations of these differences are recorded in table 4 for the 545 military White males and also for all available bone pairs of the military Negro males. The differences between the two races are in the same direction for any given bone except the humerus. The differences are all significantly different from zero for the larger sample (Whites), whereas the differences only for radius, ulna, and femur are significant for the smaller sample (Negroes). As has been found previously (Pearson, 1899; Telkkä, '50; Dupertuis and Hadden, '51) these differences are small on the average (the highest being 0.3 cm for the radius of the Negroes) although in some individual pairs they may be as much or more than 0.5 cm. It is impossible to predict the

TABLE 3

Intercorrelations among lengths of right and left bones and stature (of 545 military White males)

		MEAN (cm)	S.D. (cm)	HUM	RAD		ULNA		FEM		FEM$_m$		TIB$_m$		TIB		FIB		STATURE
				L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	
Hum	R	33.640	1.691	.976	.771	.820	.793	.791	.847	.842	.848	.838	.822	.827	.828	.829	.820	.820	.783
	L	33.595	1.672	...	.771	.826	.794	.798	.842	.837	.843	.834	.824	.828	.832	.832	.827	.829	.788
Rad	R	25.243	1.338	...	...	.925	.913	.898	.726	.726	.728	.722	.796	.799	.803	.799	.807	.803	.720
	L	25.058	1.271	...	...	...	.946	.967	.774	.775	.776	.770	.842	.849	.851	.850	.856	.855	.764
Ulna	R	27.131	1.302	...	...	...	...	.968	.758	.754	.761	.750	.832	.835	.840	.837	.850	.845	.751
	L	26.938	1.285						.754	.751	.757	.747	.830	.835	.840	.838	.849	.847	.747
Fem	R	46.853	2.315	...	...	...	...	...	...	.985	.997	.978	.864	.869	.879	.878	.867	.867	.868
	L	46.964	2.316	...	...	...	...	...	...	...	.983	.993	.861	.872	.877	.882	.865	.872	.862
Fem$_m$	R	47.232	2.358	...	...	...	...	...	...	...	...	.980	.868	.871	.882	.880	.870	.870	.869
	L	47.290	2.357	...	...	...	...	...	...	...	...	...	.855	.867	.871	.876	.861	.868	.857
Tib$_m$	R	37.799	2.186	...	...	...	...	...	...	...	...	...	...	.987	.981	.970	.964	.957	.859
	L	37.854	2.187	...	...	...	...	...	...	...	...	...	...	...	.980	.989	.963	.971	.865
Tib	R	36.834	2.099	...	...	...	...	...	...	...	...	...	...	...	...	.989	.976	.970	.870
	L	36.862	2.139	...	...	...	...	...	...	...	...	...	...	...	...	...	.968	.976	.869
Fib	R	38.118	2.074	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.988	.864
	L	38.153	2.107	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.861
Stature		173.899	6.626	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

amount or direction of this difference for any particular pair in a single individual. However, the error could be corrected in an estimation equation by adjusting for the difference between the means. When the equation is based on the average length of both bones only one-half of the indicated difference between the right and left bones should be added or subtracted to the measured length for insertion in the equation. This amounts to less than a millimeter for all excepting the radius and ulna and thus is less than the error of measurement itself.

TABLE 4

Mean differences[1] (cm) (with standard errors) and standard deviations between lengths of right and left paired bones of military personnel. (The numbers in parentheses indicate the number of subjects involved)

	WHITE MALES (545)		NEGRO MALES (68–80)	
	Mean diff.	S.D.	Mean diff.	S.D.
Humerus	+ .045 ± .016	.365	— .021 ± .041	.366
Radius	+ .185 ± .023	.510	+ .300 ± .037	.323
Ulna	+ .193 ± .017	.389	+ .252 ± .039	.319
Femur	— .111 ± .018	.404	— .095 ± .045	.405
Femur_m	— .058 ± .021	.472	— .043 ± .046	.411
Tibia_m	— .055 ± .016	.359	— .054 ± .032	.282
Tibia	— .028 ± .014	.321	— .046 ± .033	.295
Fibula	— .035 ± .015	.326	— .057 ± .047	.388

[1] The difference is expressed as length of right bone minus length of left, thus a + figure signifies a longer right bone and a — figure, a longer left bone.

Even for these two bones it can easily be shown that the resultant error in the estimated stature is less than half a centimeter. It seems impractical and unnecessary, therefore, to make an adjustment when only one bone of a pair is available for stature estimation.

Equations for estimation of stature from a given bone length. For the determination of equations for estimation of stature from long bone lengths, the average length of the paired bones was utilized. Complete matrices of intercorrelations of stature, age, and long bone measurements were computed. In the military White group those cases (165) in which all but the ulna(e) and/or fibula(e) were present were treated separately from the "complete" cases. (Data for the "miscellaneous incomplete" White subjects were not included in these computations.) In the military Negro group all possible bone pairs were included in each correlation with stature because of the small sample. All subjects from the Terry Collection were complete and were utilized for obtaining correlations and regression equations.

The means (with standard errors) and the standard deviations of the distribution of measurements for each group are presented in table 5. For the Terry Collection the stature is that of the cadaver whereas for the military samples the stature is that of the living, thus the averages are not directly comparable. However, all bones measured were without cartilage and dry and hence are directly comparable.

The differences between the mean measurements and between the standard deviations for the two groups of military White males are insignificant, indicating as may have been expected, uniformity of the complete and incomplete groups with respect to age, stature and average length of bone pairs.

Every measurement for White males is significantly smaller for the Terry Collection subjects than for military personnel. However, the standard deviations of these measurements for the two sources are in agreement within the limits of sampling error, excepting that of stature which is significantly greater for the Terry Collection subjects. It is suggested that part of the variance in cadaver stature has resulted from post-mortem changes which are differentially produced. In this connection it is noted that the standard deviation of stature measurements is much larger for Negro males of the Terry Collection than for Negroes of military origin. This large difference is partially due to a difference in general variability between the two samples since the Negroes of the Terry Collection have a significantly larger standard deviation for each bone measurement than do the military Negroes or any of the White samples. It is interesting also, that the Negro samples from the two sources have comparable measurements of upper limb bones, but that the measurements of the lower limb bones differ sig-

TABLE 5

Mean (with standard error) and standard deviation of age (years), stature[1] *and long bone measurements (cm) according to race, sex and source of data*

White males

	MILITARY COMPLETE (545)		MILITARY INCOMPLETE (165)		TERRY COLLECTION (255)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Age	23.14 ± .18	4.31	22.65 ± .35	4.46	61.66 ± .77	12.25
Stature	173.899 ± .284	6.626	174.442 ± .476	6.091	170.392 ± .461	7.343
Humerus	33.618 ± .072	1.672	33.678 ± .124	1.582	32.998 ± .112	1.787
Radius	25.151 ± .055	1.280	25.099 ± .103	1.316	24.403 ± .084	1.334
Ulna	27.035 ± .055	1.283			26.218 ± .088	1.402
Femur	46.908 ± .099	2.306	47.179 ± .187	2.391	45.415 ± .151	2.411
Femur$_m$	47.261 ± .100	2.346	47.525 ± .188	2.410	45.660 ± .154	2.447
Tibia$_m$	37.826 ± .093	2.179	37.991 ± .181	2.316	36.374 ± .136	2.170
Tibia	36.848 ± .091	2.113	37.059 ± .180	2.307	35.345 ± .134	2.139
Fibula	38.135 ± .089	2.084			36.782 ± .132	2.103

Negro males

	MILITARY (54)		TERRY COLLECTION (360)	
	Mean	S.D.	Mean	S.D.
Age	25.07 ± .68	4.98	49.46 ± .82	15.51
Stature	172.111 ± .843	6.139	172.729 ± .412	7.807
Humerus	33.793 ± .184	1.337	33.777 ± .099	1.883
Radius	26.568 ± .170	1.240	26.322 ± .084	1.597
Ulna	28.509 ± .182	1.323	28.164 ± .086	1.623
Femur	47.930 ± .307	2.234	47.073 ± .153	2.903
Femur$_m$	48.337 ± .310	2.256	47.424 ± .157	2.969
Tibia$_m$	39.554 ± .316	2.298	38.721 ± .134	2.533
Tibia	38.606 ± .322	2.344	37.667 ± .131	2.486
Fibula	39.763 ± .315	2.295	38.950 ± .130	2.456

Females (Terry Collection)

	WHITE (63)		NEGRO (177)	
	Mean	S.D.	Mean	S.D.
Age	63.93 ± 2.02	16.07	47.21 ± 1.55	17.64
Stature	160.682 ± .946	7.508	160.892 ± .574	6.534
Humerus	30.430 ± .218	1.728	30.764 ± .139	1.578
Radius	22.211 ± .156	1.240	23.602 ± .130	1.477
Ulna	23.994 ± .173	1.372	25.390 ± .115	1.305
Femur	42.654 ± .315	2.503	43.273 ± .205	2.335
Femur$_m$	42.959 ± .319	2.531	43.712 ± .210	2.391
Tibia$_m$	34.029 ± .271	2.151	35.415 ± .188	2.135
Tibia	33.181 ± .263	2.091	34.538 ± .184	2.098
Fibula	34.335 ± .270	2.143	35.549 ± .184	2.099

[1] Stature indicates measurement of the living for military personnel and of the cadaver for the Terry Collection subjects.

nificantly, the Negro sample from the Terry Collection having the shorter bones.

The two female samples from the Terry Collection, which are directly comparable with regard to stature, differ mainly in the lengths of the bones of the forearm and leg (radius, ulna, tibia and fibula) although for every bone the Negro female has a longer average length than the White female.

The average age of the samples from the Terry Collection is much older than the age of the samples of military origin and the spread is considerably wider extending into the later decades. The coefficient of correlation of each measurement with age for the Terry Collection samples is presented in table 6. It can be seen that stature and age are negatively correlated. This negative relationship is contributed to by effects on stature of both the secular trend and ageing. For the secular trend it has been shown that the older the individual the less likely he is to have attained as tall a stature as younger individuals living in the same period. And, for ageing it has been shown that the older the individual (after 30 years of age) the greater will have been his loss of stature (Trotter and Gleser, '51a,b). The effect of the secular trend on stature is evidenced by the negative correlation between

TABLE 6

Coefficients of correlation of age with stature and with long bone measurements for the Terry Collection samples, according to sex and race

	MALES		FEMALES	
	White	Negro	White	Negro
Stature	—.09	—.24	—.31	—.20
Humerus	.03	—.08	—.14	—.02
Radius	.00	—.14	—.02	—.07
Ulna	.01	—.12	.06	.05
Femur	—.01	—.11	—.11	—.09
Femur$_m$	—.02	—.12	—.12	—.11
Tibia$_m$	.04	—.15	—.05	—.07
Tibia	.04	—.15	—.08	—.09
Fibula	.04	—.15	—.05	—.09

age and length of most of the bones. After eliminating this effect by a partial correlation technique, the correlation between stature and age was still negative and statistically significant and was found to be homogeneous for the 4 groups amounting to an average rate of decline of .06 cm per year after 30 years of age (see earlier study). This indicates that the additive constant of an equation for estimation of stature from long bone lengths would vary according to the age of the individual. Thus, a more accurate estimation of stature can be made by including in the calculation an adjustment for the effect of ageing.

The coefficients of correlation between stature and each of the long bone lengths for the several samples of White and Negro subjects are presented in table 7. For the Terry Collection samples the partial correlations of stature with bone length, when age is held statistically constant, are also indicated in parentheses. The standard errors of the correlation coefficients are included to permit determination of significant differences between the corresponding correlations in different samples by inspection. The correlations between stature and long bone lengths for the two military White samples differ only to an extent which might be expected from sampling fluctuations. Since the differences in the means and standard errors of each measurement were insignificant also, it can be concluded that these two samples are drawn from the same population and that equations for estimation of stature from their long bones would not differ significantly.

The two military samples of White males (710 subjects) were, therefore, combined and the means, standard deviations, and correlation coefficients were computed for the total sample using as many data as were available for the ulna and fibula (table 8). (Data for the ulna and fibula were included from the 165 ''incomplete'' cases when these bone pairs were present.) The differences in correlations of stature and long bone lengths between the military and the Terry Collection White samples are not significant (see tables 7 and 8). The correlations of the latter group are all somewhat lower, especially those for

TABLE 7

Coefficients of correlation (with standard errors) between stature and long bone measurements according to race, sex, and source. Partial correlations when age is held statistically constant are also shown in parentheses for the Terry Collection subjects

	White			
	MALE			FEMALE
	Military personnel Complete (545)	Military personnel Incomplete (165)	Terry Collection (255)	Terry Collection (63)
Humerus	.790 ± .016	.754 ± .034	.751 ± .027 (.757)	.802 ± .045 (.806)
Radius	.756 ± .018	.732 ± .036	.730 ± .029 (.732)	.789 ± .048 (.823)
Ulna	.755 ± .018		.726 ± .030 (.731)	.759 ± .053 (.731)
Femur	.869 ± .010	.858 ± .021	.859 ± .016 (.862)	.851 ± .035 (.864)
Femur$_{m}$	.867 ± .011	.861 ± .020	.861 ± .016 (.863)	.858 ± .033 (.869)
Tibia$_{m}$	.864 ± .011	.833 ± .024	.818 ± .031 (.826)	.845 ± .036 (.873)
Tibia	.872 ± .010	.837 ± .023	.816 ± .023 (.825)	.841 ± .037 (.861)
Fibula	.865 ± .011		.814 ± .021 (.822)	.851 ± .035 (.879)
	Negro			
	(68–80)		(360)	(177)
Humerus	(79) .716 ± .055		.821 ± .017 (.828)	.748 ± .017 (.759)
Radius	(74) .713 ± .058		.792 ± .020 (.789)	.633 ± .053 (.634)
Ulna	(68) .712 ± .060		.773 ± .021 (.772)	.649 ± .051 (.673)
Femur	(80) .769 ± .046		.817 ± .018 (.820)	.835 ± .027 (.837)
Femur$_{m}$	(80) .768 ± .046		.818 ± .017 (.818)	.848 ± .025 (.853)
Tibia$_{m}$	(79) .803 ± .038		.859 ± .014 (.857)	.811 ± .030 (.824)
Tibia	(79) .799 ± .041		.857 ± .014 (.855)	.809 ± .030 (.810)
Fibula	(68) .766 ± .050		.861 ± .014 (.859)	.813 ± .030 (.814)

TABLE 8

Means (standard errors), standard deviations, and coefficients of correlation (with standard errors) of stature and long bone measurements (cm) of military Whites males[1]

	MEAN	S.D.	r
Stature	174.035 ± .244	6.510	
Humerus	33.632 ± .062	1.653	.783 ± .015
Radius	25.139 ± .048	1.287	.748 ± .017
Ulna	27.024 ± .052	1.317	.749 ± .016
Femur	46.971 ± .087	2.328	.865 ± .009
Femur$_{m}$	47.322 ± .089	2.365	.865 ± .009
Tibia$_{m}$	37.865 ± .083	2.214	.856 ± .010
Tibia	36.897 ± .082	2.162	.862 ± .010
Fibula	38.153 ± .087	2.095	.863 ± .010

[1] The statistics are based for ulna on 644 cases, for fibula on 580 cases, and for all other values on 710 cases.

the tibia and fibula. The differences in correlations between the Negro males from the two sources (table 7) are also not significant. It may be concluded, therefore, that the two samples are drawn from populations equally correlated with regard to stature and length of long bones. The correlations of stature with long bone lengths for the Terry Collection subjects are, in general, slightly higher when age is held statistically constant (table 7) than when age is allowed to vary. The differences are very slight (as predicted by Pearson) and would have very little effect on the slope of the line of regression for stature estimation from long bone length. However, the additive constant of the equation, as has already been noted, is different for different age groups.

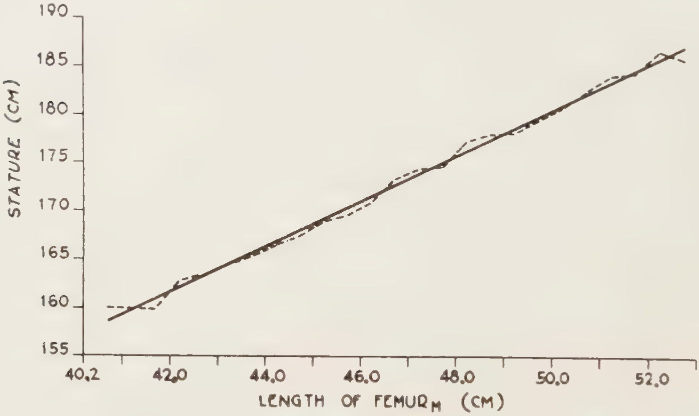

Fig. 1 Regression line and mean statures of 710 military White males grouped according to increments of 0.5 cm in length of femur$_m$.

The possibility was considered that a more accurate estimation of stature from the length of long bones might be obtained by utilizing other than a linear relationship. To test this possibility the mean statures corresponding to increments of 0.5 cm in the length of the femur$_m$ were computed for the 710 military White males. The resulting averages are shown in figure 1 together with the best fitting line of regression. The correlation ratio for this bivariate distribution, arranged in arrays, is .869 as compared to the correlation coefficient of .865.

This difference is not significant statistically. It may be concluded that the relationship between stature and length of femur is linear and that no advantage would be gained in using other than a linear relationship in estimating stature from the femur_m length. Also, there is no reason to doubt that linearity of regression is obtained for stature when estimated from each of the other bones. In point of fact Breitinger proved linearity of regression between stature and length of the radius for his sample. Thus, it has been established empirically that the Pearsonian method of linear regression is justified.

The best fitting linear equation for estimation of stature from the length of each long bone was obtained for each of the samples (table 9). These equations are for estimation of maxi-

TABLE 9

Equations for estimation of stature (cm)[1] *(with standard errors) from long bone lengths according to race, sex and source*

White		
MALE Military personnel (Living stature)	MALE Terry Collection (Cadaver stature)	FEMALE Terry Collection (Cadaver stature)
3.08 Hum + 70.45 ± 4.05	3.10 Hum + 70.00 ± 4.78	3.36 Hum + 60.47 ± 4.45
3.78 Rad + 79.01 ± 4.32	4.01 Rad + 74.43 ± 4.97	4.74 Rad + 57.43 ± 4.24
3.70 Ulna + 74.05 ± 4.32	3.81 Ulna + 72.40 ± 4.99	4.27 Ulna + 60.26 ± 4.30
2.42 Fem + 60.37 ± 3.27	2.61 Fem + 53.76 ± 3.69	2.48 Fem + 56.93 ± 3.78
2.38 Fem_m + 61.41 ± 3.27	2.58 Fem_m + 54.79 ± 3.69	2.47 Fem_m + 56.60 ± 3.72
2.52 Tib_m + 78.62 ± 3.37	2.79 Tib_m + 70.81 ± 4.13	2.90 Tib_m + 64.03 ± 3.66
2.60 Tib + 78.10 ± 3.30	2.82 Tib + 72.62 ± 4.15	2.95 Tib + 64.83 ± 3.82
2.68 Fib + 71.78 ± 3.29	2.86 Fib + 67.09 ± 4.17	2.93 Fib + 62.11 ± 3.57
Negro		
3.26 Hum + 62.10 ± 4.43	3.35 Hum + 60.75 ± 4.39	3.08 Hum + 67.17 ± 4.25
3.42 Rad + 81.56 ± 4.30	3.78 Rad + 74.40 ± 4.79	2.75 Rad + 97.01 ± 5.05
3.26 Ulna + 79.29 ± 4.42	3.63 Ulna + 71.66 ± 4.96	3.31 Ulna + 77.88 ± 4.83
2.14 Fem + 69.74 ± 3.93	2.15 Fem + 72.69 ± 4.47	2.30 Fem + 62.39 ± 3.58
2.11 Fem_m + 70.35 ± 3.94	2.11 Fem_m + 73.84 ± 4.49	2.28 Fem_m + 62.26 ± 3.41
2.19 Tib_m + 86.02 ± 3.78	2.60 Tib_m + 73.23 ± 4.02	2.45 Tib_m + 75.15 ± 3.70
2.17 Tib + 88.83 ± 3.82	2.64 Tib + 74.46 ± 4.05	2.48 Tib + 76.27 ± 3.83
2.19 Fib + 85.65 ± 4.08	2.68 Fib + 69.51 ± 4.00	2.49 Fib + 73.40 ± 3.80

[1] The stature obtained in each case is that of an individual of 18 to 30 years of age. For the stature of older individuals subtract .06 (age – 30) cm to obtain stature at desired age.

mum stature. An adjustment has been made, where necessary, to offset the effect of ageing on stature. This adjustment for age was not necessary for the military personnel but for the Terry Collection subjects the additive constant was corrected to give the best estimate of maximum stature. When a stature estimate is desired for individuals above 30 years of age, the equation should be modified by subtracting from the estimate the factor, .06 (age — 30) cm. Thus, to obtain the original cadaver stature of each of the Terry Collection samples, the average age of the sample should be inserted in the above expression and the resulting value subtracted from the estimates obtained from the equations as listed in table 9.

It should be emphasized again that the estimation equations for the military personnel provide an estimate of living stature whereas those for the Terry Collection subjects, an estimate of cadaver stature. All equations are to be applied to measurements of dry bones without cartilage. For the military Negro males the equations were computed using the means and standard deviations of stature and long bone length of all individuals for which the particular bone pair was available. Since these values vary somewhat from those listed in table 5 for "complete" military Negro males the detailed statistics are given:

BONE	NO. OF SUBJECTS	BONE LENGTH		STATURE	
		Mean	S.D.	Mean	S.D.
Humerus	79	33.757	1.392	172.151	6.349
Radius	74	26.443	1.279	172.000	6.140
Ulna	68	28.426	1.377	171.956	6.309
Femur	80	47.845	2.209	172.125	6.153
Femur_m	80	48.235	2.246	172.125	6.153
Tibia_m	79	39.485	2.323	172.494	6.342
Tibia	79	38.554	2.331	172.494	6.342
Fibula	68	39.799	2.219	172.809	6.346

In every set of equations, it can be seen that stature has a smaller standard error of estimate when computed from bones of the lower limb than when computed from bones of the upper limb. Thus, the femur, the tibia or the fibula give the best estimates of stature for each group. The two different measurements of the femur and of the tibia in every group give practically identical equations except for the constant term which reflects the difference in average lengths obtained from the two measurements. In general the slopes of the regression equations for stature obtained from the military personnel and from the Terry Collection samples differ only to the extent expected by sampling. It is rather interesting to note, however, that the standard error of estimate is smaller for the military personnel than for the Terry Collection subjects for every comparable equation, indicating again that living stature measurements introduce less error variance.

Multiple regression equations for estimation of stature. When the intercorrelations among several independent variables are known it is possible to determine by multiple regression techniques the best fitting linear equation using any number of these variables in combination. In other words, coefficients can be obtained so that the correlation of $ax_1 + bx_2 + cx_3 + \ldots gx_n + h$ with the dependent variable will be a maximum. This method was introduced by Pearson. It includes, as special cases, the possibility that the variables be given equal weight (in which case the estimates obtained from the different variables may be simply averaged) and also the possibility that all but one of the variables have negligible weights (in which case only one variable need be used for obtaining the best estimation). The actual weights or coefficients obtained for the variables will, of course, vary from sample to sample of a population depending upon the obtained matrix of intercorrelations.

Since the two measurements of the femur and of the tibia give practically identical results in each case for the estimation of stature and since each is almost perfectly correlated with the other there is no advantage in using both measurements of either bone in the determination of multiple equations of estimation. Arbitrarily the measurements indicated as femur_m and tibia_m were retained. The complete matrices of intercorrelations among lengths of the 6 long bones for each of the samples are presented in table 10. In every sample the correlations among the bones are very high indicating that little additional precision can be gained from a multiple regression equation. In particular, the correlations between radius and ulna and between tibia and fibula (both ranging

TABLE 10

Intercorrelations among long bone measurements and stature according to race, sex and source

White males

	MILITARY PERSONNEL (710)[1]						TERRY COLLECTION (255)					
	Hum	Rad	Ulna	Fem$_m$	Tib$_m$	Fib	Hum	Rad	Ulna	Fem$_m$	Tib$_m$	Fib
Stat	.783	.748	.749	.865	.856	.863	.751	.730	.726	.861	.818	.814
Hum	...	.829	.805	.843	.828	.832	...	.838	.819	.853	.827	.836
Rad	.829	...	.956	.776	.850	.848	.838	...	.970	.799	.863	.873
Ulna	.805	.956	...	.764	.843	.858	.819	.970	...	.796	.852	.868
Fem$_m$	.843	.776	.764	...	.880	.874	.853	.799	.796	...	.890	.884
Tib$_m$	.828	.850	.843	.880	...	.970	.827	.863	.852	.890	...	.975
Fib	.832	.848	.858	.874	.970	...	.836	.873	.868	.884	.975	.

Negro males

	MILITARY PERSONNEL (54)						TERRY COLLECTION (360)					
Stat	.701	.649	.643	.758	.813	.793	.821	.792	.773	.818	.859	.861
Hum	...	.726	.676	.763	.759	.749	...	.832	.813	.828	.858	.860
Rad	.726	...	.961	.696	.852	.836	.832	...	.967	.773	.866	.880
Ulna	.676	.961	...	.677	.820	.812	.813	.967	...	.752	.850	.864
Fem$_m$	.763	.696	.677	...	.849	.831	.828	.773	.752	...	.848	.854
Tib$_m$	.759	.852	.820	.849	...	.974	.858	.866	.850	.848	...	.980
Fib	.749	.836	.812	.831	.974	...	.860	.880	.864	.854	.980	.

Terry Collection females

	WHITE (63)						NEGRO (177)					
Stat	.802	.789	.759	.858	.845	.851	.748	.633	.649	.848	.811	.813
Hum	...	.833	.794	.875	.834	.837	...	.722	.804	.819	.832	.832
Rad	.833	...	.963	.874	.878	.878	.722	...	.824	.719	.737	.758
Ulna	.794	.963	...	.838	.876	.885	.804	.824	...	.718	.799	.820
Fem$_m$	.875	.874	.838	...	.905	.910	.819	.719	.718	...	.870	.880
Tib$_m$	.834	.878	.876	.905	...	.983	.832	.737	.799	.870	...	.980
Fib	.837	.878	.885	.910	.983	...	.832	.758	.820	.880	.980	...

[1] The statistics are based for ulna on 644 cases, for fibula on 580 cases, and for all other values on 710 cases.

above .95 in every group except the Negro females) indicate that there is no advantage in using both bones. Since the ulna and fibula are broken or missing more frequently than the radius and tibia among skeletal remains, they were eliminated from the computation of multiple regression equations.

Multiple regression equations of stature with the lengths of two or more bones in various combinations (humerus, radius, femur_m and tibia_m) for each sample are presented in table 11. This table reveals many interesting facts. There is no perceptible increase in accuracy of estimation obtained from using measurements of all 4 bones over that obtained from using two selected bones (femur_m and tibia_m). The Negro male groups constitute a possible exception, but, even in these, the gain in correlation is so slight that it could not be expected to hold for a new sample in which these same regression weights are utilized. This is evident in the fact that the radius presents a large negative weight in the military Negro group whereas its weight becomes a small positive value in the comparable Negro group of the Terry Collection. There appears, therefore, to be no advantage in using lengths of all 4 bones simultaneously for estimating stature. Especially should the practice of giving equal weight to all 4 bones, by averaging together the estimations derived from each one, be discouraged. That such a procedure leads to less valid estimates is obvious when the relative weights of the various bone measurements in equation (1) for each group are noted. For the White male and female groups and for the Negro females it is evident that the humerus and radius add little or nothing to the accuracy of estimation when the femur and tibia are available. For all these three groups equally valid estimates are obtained from equations (6 or 7) which involve only the femur and tibia. Also, these are simpler equations to apply than those utilizing lengths of three or 4 bones. Finally, equation (7) utilizing the sum of the lengths of femur and tibia gives a result in every group of nearly, if not, the maximum validity. In no estimation of stature should the humerus and radius be used separately or in conjunction with each other (equation 4) if the

TABLE 11

Multiple regression equations for estimation of stature (cm)[1] *(with standard errors) and coefficients of multiple correlation (R) from long bone lengths according to race, sex and source*

		R
	Military personnel — White males (living stature)	
(1)	$0.28\ \text{Hum} - 0.02\ \text{Rad} + 1.32\ \text{Fem}_m + 1.16\ \text{Tib}_m + 58.73 \pm 2.99$	.888
(2)	$0.27\ \text{Hum} + 1.32\ \text{Fem}_m + 1.16\ \text{Tib}_m + 58.57 \pm 2.99$	.889
(3)	$0.37\ \text{Hum} + 0.77\ \text{Rad} + 1.84\ \text{Fem}_m + 55.16 \pm 3.15$	.875
(4)	$2.05\ \text{Hum} + 1.60\ \text{Rad} + 64.86 \pm 3.88$	.803
(5)	$0.93\ \text{Hum} + 1.94\ \text{Tib}_m + 69.30 \pm 3.26$	.866
(6)	$1.42\ \text{Fem}_m + 1.24\ \text{Tib}_m + 59.88 \pm 2.99$	.888
(7)	$1.30(\text{Fem}_m + \text{Tib}_m) + 63.29 \pm 2.99$	.888
	Terry Collection — White males (cadaver stature)[2]	
(1)	$0.03\ \text{Hum} + 0.03\ \text{Rad} + 1.82\ \text{Fem}_m + 0.92\ \text{Tib}_m + 54.01 \pm 3.58$	.873
(2)	$0.04\ \text{Hum} + 1.82\ \text{Fem}_m + 0.93\ \text{Tib}_m + 54.04 \pm 3.58$	.873
(3)	$0.03\ \text{Hum} + 0.63\ \text{Rad} + 2.28\ \text{Fem}_m + 51.81 \pm 3.66$	.867
(4)	$1.97\ \text{Hum} + 1.81\ \text{Rad} + 63.10 \pm 4.60$	.780
(5)	$0.85\ \text{Hum} + 2.24\ \text{Tib}_m + 62.76 \pm 3.96$	.842
(6)	$1.84\ \text{Fem}_m + 0.94\ \text{Tib}_m + 54.08 \pm 3.58$	.873
(7)	$1.40(\text{Fem}_m + \text{Tib}_m) + 57.43 \pm 3.69$	.865
	Military personnel — Negro males (living stature)	
(1)	$0.89\ \text{Hum} - 1.01\ \text{Rad} + 0.38\ \text{Fem}_m + 1.92\ \text{Tib}_m + 74.56 \pm 3.38$	.835
(2)	$0.67\ \text{Hum} + 0.49\ \text{Fem}_m + 1.47\ \text{Tib}_m + 67.64 \pm 3.44$	.828
(3)	$1.02\ \text{Hum} + 0.75\ \text{Rad} + 1.32\ \text{Fem}_m + 53.91 \pm 3.78$	.788
(4)	$2.23\ \text{Hum} + 1.47\ \text{Rad} + 57.70 \pm 4.20$	.730
(5)	$0.90\ \text{Hum} + 1.78\ \text{Tib}_m + 71.29 \pm 3.49$	.823
(6)	$0.66\ \text{Fem}_m + 1.62\ \text{Tib}_m + 76.13 \pm 3.49$	.823
(7)	$1.15(\text{Fem}_m + \text{Tib}_m) + 71.04 \pm 3.53$	.818
	Terry Collection — Negro males (cadaver stature)[2]	
(1)	$0.95\ \text{Hum} + 0.35\ \text{Rad} + 0.60\ \text{Fem}_m + 1.20\ \text{Tib}_m + 57.68 \pm 3.54$	.891
(2)	$1.05\ \text{Hum} + 0.60\ \text{Fem}_m + 1.32\ \text{Tib}_m + 54.67 \pm 3.54$	.891
(3)	$1.36\ \text{Hum} + 1.10\ \text{Rad} + 0.93\ \text{Fem}_m + 54.90 \pm 3.74$	.878
(4)	$2.25\ \text{Hum} + 1.56\ \text{Rad} + 56.84 \pm 4.02$	.857
(5)	$1.42\ \text{Hum} + 1.68\ \text{Tib}_m + 60.89 \pm 3.66$	.883
(6)	$0.84\ \text{Fem}_m + 1.76\ \text{Tib}_m + 65.91 \pm 3.67$	.882
(7)	$1.26(\text{Fem}_m + \text{Tib}_m) + 65.36 \pm 3.77$	.876
	Terry Collection — White females (cadaver stature)[2]	
(1)	$0.68\ \text{Hum} - 0.04\ \text{Rad} + 1.18\ \text{Fem}_m + 1.16\ \text{Tib}_m + 52.74 \pm 3.51$	.884
(2)	$0.68\ \text{Hum} + 1.17\ \text{Fem}_m + 1.15\ \text{Tib}_m + 52.62 \pm 3.51$	.884
(3)	$0.80\ \text{Hum} + 0.65\ \text{Rad} + 1.71\ \text{Fem}_m + 50.47 \pm 3.66$	.873
(4)	$1.99\ \text{Hum} + 2.31\ \text{Rad} + 50.85 \pm 4.04$	.843
(5)	$1.35\ \text{Hum} + 1.95\ \text{Tib}_m + 55.27 \pm 3.67$	.872
(6)	$1.48\ \text{Fem}_m + 1.28\ \text{Tib}_m + 55.57 \pm 3.55$	.881
(7)	$1.39(\text{Fem}_m + \text{Tib}_m) + 55.70 \pm 3.55$	.881
	Terry Collection — Negro females (cadaver stature)[2]	
(1)	$0.44\ \text{Hum} - 0.20\ \text{Rad} + 1.46\ \text{Fem}_m + 0.86\ \text{Tib}_m + 58.83 \pm 3.22$	.870
(2)	$0.39\ \text{Hum} + 1.43\ \text{Fem}_m + 0.82\ \text{Tib}_m + 58.37 \pm 3.22$	.870
(3)	$0.80\ \text{Hum} - 0.01\ \text{Rad} + 1.85\ \text{Fem}_m + 56.68 \pm 3.29$	.864
(4)	$2.57\ \text{Hum} + 0.76\ \text{Rad} + 64.92 \pm 4.08$	.781
(5)	$1.08\ \text{Hum} + 1.79\ \text{Tib}_m + 65.30 \pm 3.58$	.836
(6)	$1.53\ \text{Fem}_m + 0.96\ \text{Tib}_m + 61.04 \pm 3.23$	.869
(7)	$1.26(\text{Fem}_m + \text{Tib}_m) + 62.22 \pm 3.28$	.865

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[1] For estimation of the stature of individuals above 30 years of age subtract .06 (age − 30) cm from the derived estimates.

[2] Corrected for age to estimate maximum cadaver stature.

other bones are available, since the bones of the upper limb result in greater errors of estimate than the bones of the lower limb.

Estimation of long bone lengths from femur$_m$. The intercorrelations among the various bone lengths provide the necessary statistics for constructing estimation equations for any bone length in terms of the length of another bone. Such equations make possible the comparison of various populations insofar as the relationship among their bone lengths is concerned. Pearson has compared Naqada, Aino and French samples by such formulae. Comparisons have been made also (Dupertuis and Hadden) by using ratios of mean bone lengths, such as, tibia/femur or radius/humerus. These ratios imply a linear relationship of the form, $y = ax$. However, the general best fitting linear equation is of the form, $y = ax + b$. Unless b is negligible the ratio varies for different values of x. Thus, for samples which differ in the average length of the reference bone (x) it is possible to obtain a different ratio even though

TABLE 12

Equations for estimation of length of long bones (cm) from the length of the femur$_m$ *(with standard errors) according to source, race and sex*

Military personnel	
WHITE MALE	NEGRO MALE
$\text{Hum} = .61\ \text{Fem}_m + 4.79 \pm 0.88$	$\text{Hum} = .45\ \text{Fem}_m + 12.04 \pm 0.86$
$\text{Rad} = .42\ \text{Fem}_m + 5.30 \pm 0.82$	$\text{Rad} = .38\ \text{Fem}_m + 8.20 \pm 0.89$
$\text{Ulna} = .42\ \text{Fem}_m + 7.18 \pm 0.83$	$\text{Ulna} = .40\ \text{Fem}_m + 9.17 \pm 0.97$
$\text{Tib}_m = .81\ \text{Fem}_m - 0.45 \pm 1.06$	$\text{Tib}_m = .86\ \text{Fem}_m - 2.02 \pm 1.21$
$\text{Fib} = .78\ \text{Fem}_m + 1.27 \pm 1.01$	$\text{Fib} = .85\ \text{Fem}_m - 1.32 \pm 1.28$
Terry Collection	
WHITE FEMALE	NEGRO FEMALE
$\text{Hum} = .60\ \text{Fem}_m + 4.65 \pm 0.84$	$\text{Hum} = .54\ \text{Fem}_m + 7.16 \pm 0.91$
$\text{Rad} = .43\ \text{Fem}_m + 3.74 \pm 0.60$	$\text{Rad} = .44\ \text{Fem}_m + 4.37 \pm 1.03$
$\text{Ulna} = .45\ \text{Fem}_m + 4.66 \pm 0.75$	$\text{Ulna} = .39\ \text{Fem}_m + 8.34 \pm 0.91$
$\text{Tib}_m = .77\ \text{Fem}_m + 0.95 \pm 0.91$	$\text{Tib}_m = .78\ \text{Fem}_m + 1.32 \pm 1.05$
$\text{Fib} = .77\ \text{Fem}_m + 1.26 \pm 0.89$	$\text{Fib} = .77\ \text{Fem}_m + 1.89 \pm 1.00$

the same estimation equation of y from x would pertain to both samples.

The length of femur_m has been chosen arbitrarily as the reference bone length from which estimation equations of other bone lengths for each of the samples are obtained. The resultant equations and the corresponding standard errors of estimate are presented in table 12 for the military White and Negro male samples and for the White and Negro female samples from the Terry Collection. By inserting the average length of femur_m for a different sample into these formulae and comparing the resultant estimate with the actual average length, it is possible to determine whether or not this difference is larger than might be expected in random sampling from the same population, provided the method of measuring is the same.

The equations for estimation of lengths (cm) of various bones based on the military personnel (table 12) were applied to the males of the Terry Collection with the following results:

	WHITE MALE		NEGRO MALE	
	Length (cm)		Length (cm)	
	Estimated	Observed	Estimated	Observed
Humerus	32.64	33.00	33.38	33.78
Radius	24.48	24.40	26.22	26.32
Ulna	26.36	26.22	28.14	28.16
Tibia_m	36.53	36.37	38.76	38.72
Fibula	36.88	36.78	38.99	38.95

The estimated bone lengths are in very close agreement with the observed average bone lengths for these samples of the Terry Collection except for the humerus. Thus it appears that the male samples of the Terry Collection differ from the military personnel in having humeri that are relatively longer, but that the other bones from these two sources are quite comparable.

Comparison of equations for estimation of stature derived from military personnel with those derived from the Terry

Collection subjects. Data from the subjects of the military personnel and the Terry Collection have been treated so far in this study by parallel methods. This has made possible a comparison between the equations resulting from the two sources, and, an appraisal of the applicability of the equations derived from the military personnel to American White and Negro males of different age and socio-economic status. It is likewise believed that the comparison provides evidence for an evaluation of the equations for the females of both races, which of necessity have been determined only from data of the Terry Collection.

In order to compare directly the estimates of stature obtained from the military personnel and from the subjects of the Terry Collection it is necessary to take into account the difference between statures measured on the living and on the cadaver. As has been indicated, the equations have already been extended to cover the effect of ageing on stature by the addition of a linear factor relating stature to age.

The amount of adjustment required to convert cadaver to living stature is not considered to be the same by all investigators. Manouvrier concluded that stature measured on the cadaver was on the average 2 cm greater than if measured on the living subject. This amount of increase was utilized by Telkkä. Pearson estimated the increase to be 1.2 cm for males and 2 cm for females. On the other hand, Dupertuis and Hadden accepted the cadaver statures, which had been measured by Todd, to be in substantial agreement with the living statures. It is very likely that no one value can be applied in general but that the amount of needed correction differs according to the method used in measuring cadaver stature. In those cases where an attempt was made to determine the necessary correction it has been done on the basis of the difference between the average cadaver stature of the sample and some independent estimate of the mean stature of the total population. However, such an approach fails to take into account the fact that the mean stature of a population may have been affected by a recent secular trend indicating that a fair comparison requires means obtained from groups living in the same period. Also, this method ignores the fact that the sample of cadavers may not have been a random sample from the total population.

The average difference between cadaver and living statures for the present samples has been determined on the basis of the equations for estimation of stature for the White males from the two sources. This method is feasible since secular trends in stature have been shown to be accompanied by corresponding trends in length of long bones (Trotter and Gleser). Estimation of stature by the complete multiple regression equation for military White males should give the average living stature of White males from the Terry Collection to within an accuracy of 0.5 cm whereas the comparable equation based on the Terry Collection sample should estimate cadaver stature for the military personnel to within the same error. The average of the differences between these estimates and the recorded values, then, would approximate the difference between living and cadaver statures. The estimated cadaver stature for the White males of the military personnel is 176.725 cm (utilizing the multiple regression equation (2) in table 11,[3] based on White males of the Terry Collection) and their living stature is 174.035 cm (see table 8); the difference between the two is 2.69 cm. The living stature of the White males of the Terry Collection (utilizing the corresponding equation based on the military personnel) is 169.94 cm and the cadaver stature adjusted for age is 172.29 cm; the difference is 2.35 cm. The average correction is, therefore, 2.5 cm to one decimal place. Since it is reasonable to assume that the difference between living and cadaver stature is constant for a particular method of measurement, this same amount of correction was applied to the statures of the Negro males and to the female groups. The estimated statures for the 4 groups from the

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[3] Equation (2) was used since the weight for radius in equation (1) is essentially zero for both groups.

Terry Collection converted to living statures (cm), therefore, are as follows:

	AT AGE OF DEATH	MAXIMUM (18–30 YEARS)
White males	167.89	169.79
Negro males	170.23	171.40
White females	158.18	160.22
Negro females	158.39	159.42

In figures 2 and 3 the equations obtained from the data of White and Negro military personnel are compared to the corresponding equations from data of the Terry Collection subjects, corrected for age and living stature. For White males, substantial agreement is apparent in the stature estimates which would be obtained for any particular length of long bone throughout the range. The difference in estimate is less than 1.5 cm for all but those based on the shortest of the tibiae and radii. The equations based on the humerus give a constant difference in estimated stature, but this is not sur-

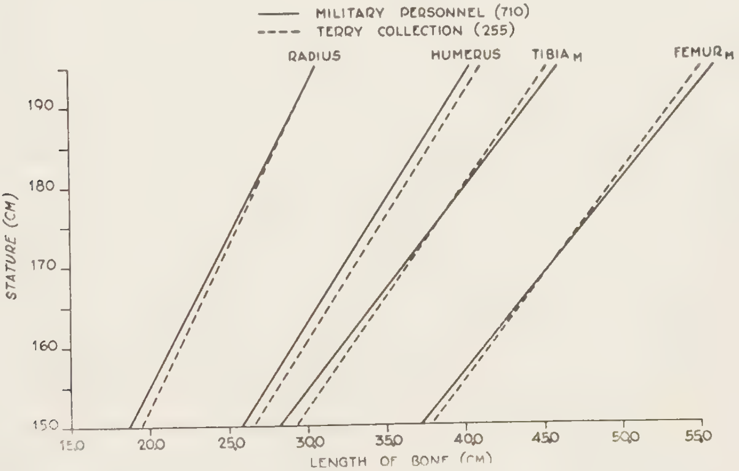

Fig. 2 Comparison of estimates of stature according to length of long bones of White males of military personnel and Terry Collection. (Data from Terry Collection subjects have been converted to maximum living stature.)

prising since it has already been noted that the average length of the humerus of White males is relatively longer for the Terry Collection than for the military personnel. It is certain that the equations based on military personnel which have been recommended for use (Tib_m, Fem_m, or the combination of the two), would estimate adequately the stature of the White males of the Terry Collection. The agreement is quite satisfactory, also, for the estimation equations for the Negroes excepting in the case of the tibia. For this bone there is some divergence in the slopes of the equations which results in a difference in stature estimate at the extremes of the slopes of approximately 3 cm. Since all the equations are so nearly alike, however, despite the limited number of cases on which the military Negro stature equations were computed, it is evident that the latter are quite adequate for estimating living stature of Negro males.

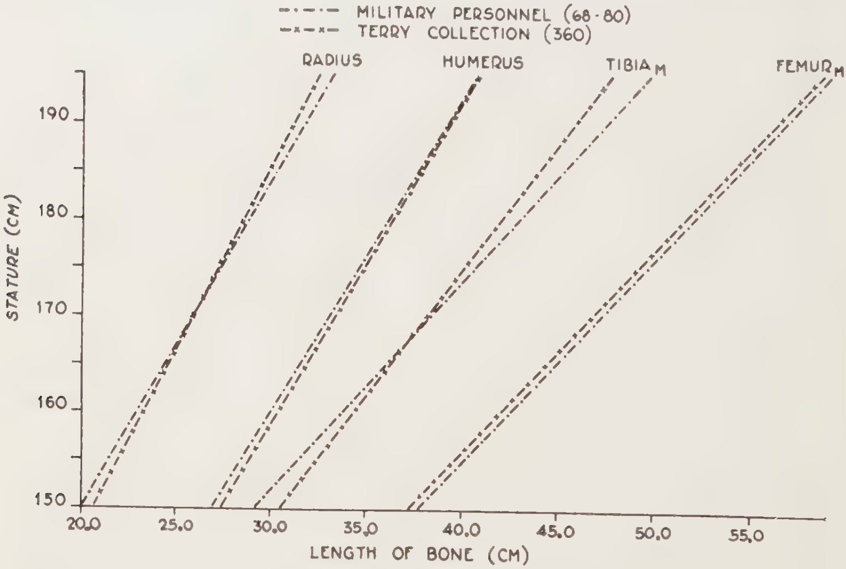

Fig. 3 Comparison of estimates of stature according to length of long bones of Negro males of military personnel and Terry Collection. (Data from Terry Collection subjects have been converted to maximum living stature.)

The final equations for estimation of living stature of Whites and Negroes of both sexes, extended to cover all ages, are presented in table 13. The equations applicable to males are from the military personnel while those for females are the corrected equations from the Terry Collection samples. Appendixes to the table present stature estimations according to a wide range of lengths of long bones for each sex of each race.

TABLE 13

Equations for estimation of living stature (cm) (with standard errors) from long bones for American Whites and Negroes between 18 and 30 years of age[1]

WHITE MALES		NEGRO MALES	
$3.08\ \mathrm{Hum} + 70.45$	± 4.05	$3.26\ \mathrm{Hum} + 62.10$	± 4.43
$3.78\ \mathrm{Rad} + 79.01$	± 4.32	$3.42\ \mathrm{Rad} + 81.56$	± 4.30
$3.70\ \mathrm{Ulna} + 74.05$	± 4.32	$3.26\ \mathrm{Ulna} + 79.29$	± 4.42
$2.38\ \mathrm{Fem}_m + 61.41$	± 3.27	$2.11\ \mathrm{Fem}_m + 70.35$	± 3.94
$2.52\ \mathrm{Tib}_m + 78.62$	± 3.37	$2.19\ \mathrm{Tib}_m + 86.02$	± 3.78
$2.68\ \mathrm{Fib} + 71.78$	± 3.29	$2.19\ \mathrm{Fib} + 85.65$	± 4.08
$1.30(\mathrm{Fem}_m + \mathrm{Tib}_m) + 63.29$	± 2.99	$1.15(\mathrm{Fem}_m + \mathrm{Tib}_m) + 71.04$	± 3.53
$1.42\ \mathrm{Fem}_m + 1.24\ \mathrm{Tib}_m + 59.88$	± 2.99	$0.66\ \mathrm{Fem}_m + 1.62\ \mathrm{Tib}_m + 76.13$	± 3.49
$0.93\ \mathrm{Hum} + 1.94\ \mathrm{Tib}_m + 69.30$	± 3.26	$0.90\ \mathrm{Hum} + 1.78\ \mathrm{Tib}_m + 71.29$	± 3.49
$0.27\ \mathrm{Hum} + 1.32\ \mathrm{Fem}_m + 1.16\ \mathrm{Tib}_m + 58.57$ [2]	± 2.99	$0.89\ \mathrm{Hum} - 1.01\ \mathrm{Rad} + 0.38\ \mathrm{Fem}_m + 1.92\ \mathrm{Tib}_m + 74.56$	± 3.38
WHITE FEMALES		NEGRO FEMALES	
$3.36\ \mathrm{Hum} + 57.97$	± 4.45	$3.08\ \mathrm{Hum} + 64.67$	± 4.25
$4.74\ \mathrm{Rad} + 54.93$	± 4.24	$2.75\ \mathrm{Rad} + 94.51$	± 5.05
$4.27\ \mathrm{Ulna} + 57.76$	± 4.30	$3.31\ \mathrm{Ulna} + 75.38$	± 4.83
$2.47\ \mathrm{Fem}_m + 54.10$	± 3.72	$2.28\ \mathrm{Fem}_m + 59.76$	± 3.41
$2.90\ \mathrm{Tib}_m + 61.53$	± 3.66	$2.45\ \mathrm{Tib}_m + 72.65$	± 3.70
$2.93\ \mathrm{Fib} + 59.61$	± 3.57	$2.49\ \mathrm{Fib} + 70.90$	± 3.80
$1.39(\mathrm{Fem}_m + \mathrm{Tib}_m) + 53.20$	± 3.55	$1.26(\mathrm{Fem}_m + \mathrm{Tib}_m) + 59.72$	± 3.28
$1.48\ \mathrm{Fem}_m + 1.28\ \mathrm{Tib}_m + 53.07$	± 3.55	$1.53\ \mathrm{Fem}_m + 0.96\ \mathrm{Tib}_m + 58.54$	± 3.23
$1.35\ \mathrm{Hum} + 1.95\ \mathrm{Tib}_m + 52.77$	± 3.67	$1.08\ \mathrm{Hum} + 1.79\ \mathrm{Tib}_m + 62.80$	± 3.58
$0.68\ \mathrm{Hum} + 1.17\ \mathrm{Fem}_m + 1.15\ \mathrm{Tib}_m + 50.12$ [2]	± 3.51	$0.44\ \mathrm{Hum} - 0.20\ \mathrm{Rad} + 1.46\ \mathrm{Fem}_m + 0.86\ \mathrm{Tib}_m + 56.33$	± 3.22

[1] To estimate stature of older individuals subtract .06 (age in years − 30) cm; to estimate cadaver stature add 2.5 cm.

[2] This equation is presented in preference to that involving the radius since the weight of the radius is essentially zero.

TABLE 13

APPENDIX 1

*Expected maximum stature * from long bone lengths (maximum) for American White males*

HUM	RAD	ULNA	STATURE		FEM	TIB	FIB	FEM + TIB
mm	*mm*	*mm*	*cm*	*in ***	*mm*	*mm*	*mm*	*mm*
265	193	211	152	59^7	381	291	299	685
268	196	213	153	60^2	385	295	303	693
271	198	216	154	60^5	389	299	307	701
275	201	219	155	61	393	303	311	708
278	204	222	156	61^3	398	307	314	716
281	206	224	157	61^6	402	311	318	723
284	209	227	158	62^2	406	315	322	731
288	212	230	159	62^5	410	319	326	738
291	214	232	160	63	414	323	329	746
294	217	235	161	63^3	419	327	333	753
297	220	238	162	63^6	423	331	337	761
301	222	240	163	64^1	427	335	340	769
304	225	243	164	64^5	431	339	344	776
307	228	246	165	65	435	343	348	784
310	230	249	166	65^3	440	347	352	791
314	233	251	167	65^6	444	351	355	799
317	235	254	168	66^1	448	355	359	806
320	238	257	169	66^4	452	359	363	814
323	241	259	170	66^7	456	363	367	821
327	243	262	171	67^3	461	367	370	829
330	246	265	172	67^6	465	371	374	837
333	249	267	173	68^1	469	375	378	844
336	251	270	174	68^4	473	379	381	852
339	254	273	175	68^7	477	383	385	859
343	257	276	176	69^2	482	386	389	867
346	259	278	177	69^5	486	390	393	874
349	262	281	178	70^1	490	394	396	882
352	265	284	179	70^4	494	398	400	889
356	267	286	180	70^7	498	402	404	897
359	270	289	181	71^2	503	406	408	905
362	272	292	182	71^5	507	410	411	912
365	275	294	183	72	511	414	415	920
369	278	297	184	72^4	515	418	419	927
372	280	300	185	72^7	519	422	422	935
375	283	303	186	73^2	524	426	426	942
378	286	305	187	73^5	528	430	430	950
382	288	308	188	74	532	434	434	957
385	291	311	189	74^3	536	438	437	965
388	294	313	190	74^6	540	442	441	973
391	296	316	191	75^2	545	446	445	980
395	299	319	192	75^5	549	450	449	988
398	302	321	193	76	553	454	452	995
401	304	324	194	76^3	557	458	456	1003
404	307	327	195	76^6	561	462	460	1010
408	309	330	196	77^1	566	466	463	1018
411	312	332	197	77^4	570	470	467	1026
414	315	335	198	78	574	474	471	1033

* The expected maximum stature should be reduced by the amount of .06 (age in years − 30) cm to obtain expected stature of individuals over 30 years of age.

** The raised number indicates the numerator of a fraction of an inch expressed in eighths, thus 59^7 should be read $59\frac{7}{8}$ inches.

TABLE 13

APPENDIX 2

*Expected maximum stature * from long bone lengths (maximum) for American Negro males*

HUM	RAD	ULNA	STATURE		FEM	TIB	FIB	FEM + TIB
mm	*mm*	*mm*	*cm*	*in ***	*mm*	*mm*	*mm*	*mm*
276	206	223	152	59^7	387	301	303	704
279	209	226	153	60^2	391	306	308	713
282	212	229	154	60^5	396	310	312	721
285	215	232	155	61	401	315	317	730
288	218	235	156	61^3	406	320	321	739
291	221	238	157	61^6	410	324	326	747
294	224	242	158	62^2	415	329	330	756
297	226	245	159	62^5	420	333	335	765
300	229	248	160	63	425	338	339	774
303	232	251	161	63^3	430	342	344	782
306	235	254	162	63^6	434	347	349	791
310	238	257	163	64^1	439	352	353	800
313	241	260	164	64^5	444	356	358	808
316	244	263	165	65	449	361	362	817
319	247	266	166	65^3	453	365	367	826
322	250	269	167	65^6	458	370	371	834
325	253	272	168	66^1	463	374	376	843
328	256	275	169	66^4	468	379	381	852
331	259	278	170	66^7	472	383	385	861
334	262	281	171	67^3	477	388	390	869
337	264	284	172	67^6	482	393	394	878
340	267	287	173	68^1	487	397	399	887
343	270	291	174	68^4	491	402	403	895
346	273	294	175	68^7	496	406	408	904
349	276	297	176	69^2	501	411	413	913
352	279	300	177	69^5	506	415	417	921
356	282	303	178	70	510	420	422	930
359	285	306	179	70^4	515	425	426	939
362	288	309	180	70^7	520	429	431	947
365	291	312	181	71^2	525	434	435	956
368	294	315	182	71^5	529	438	440	965
371	297	318	183	72	534	443	445	974
374	300	321	184	72^4	539	447	449	982
377	302	324	185	72^7	544	452	454	991
380	305	327	186	73^2	548	456	458	1000
383	308	330	187	73^5	553	461	463	1008
386	311	333	188	74	558	466	467	1017
389	314	336	189	74^3	563	470	472	1026
392	317	340	190	74^6	567	475	476	1034
395	320	343	191	75^2	572	479	481	1043
398	323	346	192	75^5	577	484	486	1052
401	326	349	193	76	582	488	490	1061
405	329	352	194	76^3	586	493	495	1069
408	332	355	195	76^6	591	498	499	1078
411	335	358	196	77^1	596	502	504	1087
414	337	361	197	77^4	601	507	508	1095
417	340	364	198	78	605	511	513	1104

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* The expected maximum stature should be reduced by the amount of .06 (age in years − 30) cm to obtain expected stature of individuals over 30 years of age.

** The raised number indicates the numerator of a fraction of an inch expressed in eighths, thus 59^7 should be read 59⅞ inches.

TABLE 13

APPENDIX 3

*Expected maximum stature * from long bone lengths (maximum) for American White females*

HUM	RAD	ULNA	STATURE		FEM	TIB	FIB	FEM + TIB
mm	*mm*	*mm*	*cm*	*in ***	*mm*	*mm*	*mm*	*mm*
244	179	193	140	55^1	348	271	274	624
247	182	195	141	55^4	352	274	278	632
250	184	197	142	55^7	356	277	281	639
253	186	200	143	56^2	360	281	285	646
256	188	202	144	56^6	364	284	288	653
259	190	204	145	57^1	368	288	291	660
262	192	207	146	57^4	372	291	295	668
265	194	209	147	57^7	376	295	298	675
268	196	211	148	58^2	380	298	302	682
271	198	214	149	58^5	384	302	305	689
274	201	216	150	59	388	305	309	696
277	203	218	151	59^4	392	309	312	704
280	205	221	152	59^7	396	312	315	711
283	207	223	153	60^2	400	315	319	718
286	209	225	154	60^5	404	319	322	725
289	211	228	155	61	409	322	326	732
292	213	230	156	61^3	413	326	329	740
295	215	232	157	61^6	417	329	332	747
298	217	235	158	62^2	421	333	336	754
301	220	237	159	62^5	425	336	340	761
304	222	239	160	63	429	340	343	768
307	224	242	161	63^3	433	343	346	776
310	226	244	162	63^6	437	346	349	783
313	228	246	163	64^1	441	350	353	790
316	230	249	164	64^5	445	353	356	797
319	232	251	165	65	449	357	360	804
322	234	253	166	65^3	453	360	363	812
324	236	256	167	65^6	457	364	366	819
327	239	258	168	66^1	461	367	370	826
330	241	261	169	66^4	465	371	373	833
333	243	263	170	66^7	469	374	377	840
336	245	265	171	67^3	473	377	380	847
339	247	268	172	67^6	477	381	384	855
342	249	270	173	68^1	481	384	387	862
345	251	272	174	68^4	485	388	390	869
348	253	275	175	68^7	489	391	394	876
351	255	277	176	69^2	494	395	397	883
354	258	279	177	69^5	498	398	401	891
357	260	282	178	70^1	502	402	404	898
360	262	284	179	70^4	506	405	407	905
363	264	286	180	70^7	510	409	411	912
366	266	289	181	71^2	514	412	414	919
369	268	291	182	71^5	518	415	418	927
372	270	293	183	72	522	419	421	934
375	272	296	184	72^4	526	422	425	941

* The expected maximum stature should be reduced by the amount of .06 (age in years − 30) cm to obtain expected stature of individuals over 30 years of age.

** The raised number indicates the numerator of a fraction of an inch expressed in eighths, thus 55^1 should be read $55\frac{1}{8}$ inches.

TABLE 13

APPENDIX 4

*Expected maximum stature * from long bone lengths (maximum) for American Negro females*

HUM	RAD	ULNA	STATURE		FEM	TIB	FIB	FEM + TIB
mm	*mm*	*mm*	*cm*	*in* **	*mm*	*mm*	*mm*	*mm*
245	165	195	140	55^{1}	352	275	278	637
248	169	198	141	55^{4}	356	279	282	645
251	173	201	142	55^{7}	361	283	286	653
254	176	204	143	56^{2}	365	287	290	661
258	180	207	144	56^{6}	369	291	294	669
261	184	210	145	57^{1}	374	295	298	677
264	187	213	146	57^{4}	378	299	302	685
267	191	216	147	57^{7}	383	303	306	693
271	195	219	148	58^{2}	387	308	310	701
274	198	222	149	58^{5}	391	312	314	709
277	202	225	150	59	396	316	318	717
280	205	228	151	59^{4}	400	320	322	724
284	209	231	152	59^{7}	405	324	326	732
287	213	235	153	60^{2}	409	328	330	740
290	216	238	154	60^{5}	413	332	334	748
293	220	241	155	61	418	336	338	756
297	224	244	156	61^{3}	422	340	342	764
300	227	247	157	61^{6}	426	344	346	772
303	231	250	158	62^{2}	431	348	350	780
306	235	253	159	62^{5}	435	352	354	788
310	238	256	160	63	440	357	358	796
313	242	259	161	63^{3}	444	361	362	804
316	245	262	162	63^{6}	448	365	366	812
319	249	265	163	64^{1}	453	369	370	820
322	253	268	164	64^{5}	457	373	374	828
326	256	271	165	65	462	377	378	836
329	260	274	166	65^{3}	466	381	382	843
332	264	277	167	65^{6}	470	385	386	851
335	267	280	168	66^{1}	475	389	390	859
339	271	283	169	66^{4}	479	393	394	867
342	275	286	170	66^{7}	484	397	398	875
345	278	289	171	67^{3}	488	401	402	883
348	282	292	172	67^{6}	492	406	406	891
352	285	295	173	68^{1}	497	410	410	899
355	289	298	174	68^{4}	501	414	414	907
358	293	301	175	68^{7}	505	418	418	915
361	296	304	176	69^{2}	510	422	422	923
365	300	307	177	69^{5}	514	426	426	931
368	304	310	178	70^{1}	519	430	430	939
371	307	313	179	70^{4}	523	434	434	947
374	311	316	180	70^{7}	527	438	438	955
378	315	319	181	71^{2}	532	442	442	963
381	318	322	182	71^{5}	536	446	446	970
384	322	325	183	72	541	450	450	978
387	325	328	184	72^{4}	545	454	454	986

* The expected maximum stature should be reduced by the amount of .06 (age in years − 30) cm to obtain expected stature of individuals over 30 years of age.

** The raised number indicates the numerator of a fraction of an inch expressed in eighths, thus 55^{1} should be read 55⅛ inches.

Test of stature estimation equations by application to a new sample. Equations obtained by curve fitting and regression techniques reflect any bias inherent in the constitution of the sample. The application of such equations to a new sample may result in an error larger than predicted by the sampling statistics. This result is not likely to occur if the original sample represented a truly random selection from the population to which the equations are subsequently applied, but the population may have been ill-defined and the sample one of convenience. For example, Pearson found that his equations (based on the French data) resulted in a poor estimation of stature for 7 French criminals. He attributed this to the bias of the second sample, but it may have been rather the bias of the original sample from the standpoint of age, socio-economic status and restricted range of statures.

In the present study among the military personnel were 368 White males with miscellaneous absences of long limb bones. These provided an opportunity for an independent check of the pertinent formulae. In this group were 100 cases for which data of the paired arm, thigh and leg bones were present. These cases have been utilized as the validation sample. Stature was estimated to the nearest centimeter according to formulae involving each of the three bones and also the formula involving length of femur$_m$ plus tibia$_m$. These estimates were compared with the statures recorded at the time of induction into military service. The range of errors and the mean error are presented in table 14 together with the percentage of statures estimated to within 3 cm of the true stature, the obtained standard error of estimate (standard deviation of estimates from true stature) and the standard error of estimate as predicted from the correlations in the original samples.

It may be seen that each of the 4 equations has resulted in an almost exact estimation of the average stature of the new sample, since the mean errors are practically zero. The obtained standard error of estimate for each equation also compares favorably to the expected standard error of estimate. For a normal distribution, the standard error of estimate provides the range of errors of approximately two-thirds of the cases. In this new sample more than two-thirds of the cases lie within such a range. In point of fact, using any of the equations except that based on the humerus, two-thirds or more of the resultant estimates deviate from the true stature by 3 cm or less. Evidently the obtained standard error of estimate is increased by a few extreme cases. For approximately 79% of American military White males the statures can be estimated to within an accuracy of 3 cm (1.2 inches) by the equation utilizing femur$_m$ plus tibia$_m$. Thus, equations based on White males of military personnel have been applied to a new sample drawn from the same population and have been shown to provide estimates of stature well within the expected range of accuracy. It has already been shown that these formulae are adequate for a sample such as that provided by the Terry Collection when the difference in age and method of measuring stature are taken into account. It can be concluded that these equations may be applied without reservation to the entire population of American White males. It would be worth while to test the formulae for Negro males and for White and Negro females, were independent samples available from the same populations. By extrapolation of evidence for the White males of military personnel it is suggested that formulae for the other three groups will provide uniformly accurate estimates when applied to the pertinent race and sex.

TABLE 14

Statistics (cm) obtained from application of selected equations for estimation of stature to a new sample of 100 military White males

EQUATION FROM TABLE 13 BASED ON	MEAN ERROR FOR GROUP	RANGE OF ERRORS OF ESTIMATE	% WITHIN 3 CM OF TRUE STATURE	OBTAINED S.E. OF ESTIMATE	EXPECTED S.E. OF ESTIMATE
Humerus	— .12	— 9 to + 9	62	3.66	4.05
Femur$_m$	— .02	— 6 to + 9	69	3.22	3.27
Tibia$_m$	00	— 7 to + 10	70	3.35	3.37
(Fem$_m$ + Tib$_m$)	+ .08	— 6 to + 9	79	3.05	2.99

Comparison of equations for estimation of stature. In addition to the formulae derived from the present samples for estimation of stature from long bones there are available also the equations and/or tables of Rollet, Manouvrier, and Pearson based on data of French males and females; of Breitinger on German males; of Telkkä on Finnish males and females; and of Dupertuis and Hadden on American Whites and Negroes of both sexes. Thus, several different populations have been studied with more or less representative samples. The question now arises as to what generalizations can be drawn regarding the suitability of any particular set of equations for a specific problem of stature estimation.

There are several aspects to consider in making comparisons among the various formulae. In the first place, it is unquestionably true that the equations for estimation of stature derived from a particular sample will provide the most accurate estimate of stature for that sample. This does not necessarily mean that they will be the best suited for the general population from which the sample was drawn since the sample may have been a biased one (i.e. not a random selection). The suitability is particularly open to question if assumptions had been necessary in the determination of the additive constant. For example, in adapting Rollet's data to living stature estimation, Pearson had to deduce the average length of dry bones from the length of humid bones; the average length of bones without cartilage from bones with cartilage; and the average stature of the living population from cadaver stature. In addition, his sample was composed almost entirely of middle-aged or old individuals averaging about 60 years. On the basis of his equations stature for a young adult population such as, for example, French military males would be estimated almost 2 cm too short due to this ageing factor alone. If an adjustment for age is made it is still doubtful that the estimates would be accurate for tall individuals because of the limited range of statures in the original sample. Evidence that Pearson's equations may not necessarily be the best for Frenchmen in general lies in his own experience of estimating the stature of 7 French criminals. The average estimate was 2.73 cm below the actual statures, whereas the equation based on the femur of the present White male sample yields an average error of only — .53 cm for the group.

Telkkä's equations suffer from similar limitations, namely, the smallness of the sample, the possible sampling bias in cadaver material, the transformation of cadaver measurements to living stature and the uncontrolled age factor. Breitinger avoided two of these difficulties by measuring a large sample of young adult living subjects but introduced the liability involved in converting measurements between palpable bony prominences on the living to measurements of dry bones. Dupertuis and Hadden utilized reasonably large samples each with an adequate range of stature excepting the White males. But their samples were drawn from the lower socio-economic level, no allowance was made for change in stature from ageing, and it was assumed that Todd's measurements of cadaver stature represent living stature. This latter assumption may be open to question in the light of experience with measurements of cadaver stature for the Terry Collection.

In applying formulae derived from data of a particular group to bone measurements from another population the possibility of differences in the relationship between bone length and stature for the two populations must be recognized. However, many workers have attributed discrepancies between estimated and observed statures to differences in the constitution of the populations involved, although much of the discrepancy may be due to differences in sampling, in methods of measurement of stature and bone lengths, and in the consequent necessary adjustments of constants. How much the differences between the various equations for estimation of stature reflect actual differences in the relationship of stature to long bone length in the national groups on which they were formulated and how much they reflect the above mentioned differences is difficult to evaluate.

It is perhaps impossible to determine which equations are best for application to skeletal remains of older races for which there are no records of actual stature. In fact, Kurth ('50) has suggested on the basis of his recent experience in estimating stature of middle Europeans of the 8th to 10th century that measurement, when possible, of the overall length of the skeletal remains *in situ* is preferable to stature estimated from the long bones according to equations based on more recent populations. On the other hand, when the question is one of determining the best equations for stature estimation in a particular population of the present era, such as the American White male, it is possible to obtain a direct answer by testing available equations on a new sample of known living stature. As already noted, such a sample of 100 American military White males is available with data of the paired arm, thigh and leg bones. The mean actual stature of this group was 173.41 cm with a standard deviation of 6.11 cm. Estimates based on the length of the humerus and femur$_m$ only were compared since slight differences exist in the methods of measuring the tibia.

In table 15 is listed the investigator, his equation and the standard error of estimate for the sample from which the equation was derived. The obtained standard error of estimate, indicated in the table, is the standard deviation of the actual statures for this sample about the line of regression. Obviously, if there is an error in the estimation of the mean stature of the new sample there will be a corresponding increment in the standard error of estimate even though the slope of the regression equation is adequate to represent the regression in the new sample. Conversely, if the slope of the equation differs considerably from the line of regression of the sample, but the means correspond, the estimate may be accurate at and near the mean, but become increasingly inaccurate for progressively shorter or taller statures. In table 15 are listed also the mean and standard deviation of errors of estimate and the range of errors for each equation in order to indicate not only the amount, but also the type, of error which is incurred in the estimation of stature for this sample. Negative deviations indicate that the estimate is smaller than the actual stature and positive values indicate the reverse. The larger the standard deviation the poorer is the fit of the slope of regression whereas the mean deviation is an indication of the constant error. Approximately two-thirds of the errors in each case lie within the range of the mean error ± the standard deviation.

TABLE 15

Errors of estimation of stature (cm) of 100 additional military White males and the obtained standard error of estimated statures according to equations (with standard errors) of certain investigators based on lengths of femur$_{\text{m}}$ *and humerus*

INVESTIGATOR	EQUATION	OBTAINED S.E. OF ESTIMATE	ERRORS OF ESTIMATION		
			Range	Mean	S.D.
	Femur$_{\text{m}}$				
Breitinger ('37)	1.64 Fem + 94.31 ± 4.8	3.93	—10 to + 6	—1.66	3.57
Dupertuis and Hadden ('51)	2.12 Fem + 77.05 ± 3.4	4.68	— 3 to + 11	+ 3.22	3.40
Manouvrier (1892)	Table	5.63	—12 to + 5	—4.38	3.53
Pearson (1899)	1.88 Fem + 81.31 ± 3.2	5.02	—11 to + 4	—3.67	3.43
Telkkä ('50)	2.10 Fem + 71.85 ± 4.9	4.28	— 9 to + 5	—2.74	3.28
Present study	2.38 Fem + 61.41 ± 3.3	3.22	— 6 to + 9	—0.02	3.22
	Humerus				
Breitinger ('37)	2.72 Hum + 83.21 ± 4.9	4.03	—10 to + 8	—1.63	3.69
Dupertuis and Hadden ('51)	2.27 Hum + 98.34 ± 4.6	4.00	— 8 to + 10	+ 0.80	3.92
Manouvrier (1892)	Table	7.07	—15 to + 7	—5.76	4.10
Pearson (1899)	2.89 Hum + 70.64 ± 3.2	7.15	—15 to + 3	—6.22	3.53
Telkkä ('50)	2.80 Hum + 75.28 ± 5.0	5.80	—12 to + 5	—4.57	3.58
Present study	3.08 Hum + 70.45 ± 4.0	3.66	— 9 to + 9	—0.12	3.65

From table 15 it is evident that the equations (based on femur and humerus) developed in this study provide a more accurate estimate of stature for American White males of military age than do the other equations that have been tested. The estimated mean stature of the group is accurate and the obtained standard error of estimation is the smallest. Manouvrier's, Pearson's and Telkkä's equations result in stature estimates which are much too low for American military males while those of Dupertuis and Hadden are too high.

A comparison of estimates between femur and humerus indicates that the range of errors in every case is smaller for the femur. Its mean error is likewise substantially smaller for all estimates except for those of Breitinger and Dupertuis and Hadden. Equations of the former give practically equivalent results for the two bones whereas those of the latter give more accurate estimates with the humerus than with the femur. The superiority of the equation for the humerus in the case of Dupertuis and Hadden is due to two compensating factors. Their subjects like those of Rollet, Telkkä, and the Terry Collection have upper limbs which are relatively longer than lower limbs when compared to the military subjects. For all such groups the estimate derived from the humerus is lower than that derived from the associated femur when applied to the military subjects. However, the estimate of mean stature from the equation of Dupertuis and Hadden based on the femur is considerably greater than the true mean, probably the result of their use of cadaver stature as equivalent to living stature. Thus, their lower estimate obtained from the humerus lies closer to the actual stature of the military group than does that obtained from the femur.

Another comparison of various equations of stature estimation was based only on the femur. The equation of each investigator has been applied (as directed by him to obtain living stature) to the mean femur length of every other sample of like sex. The application of equations obtained in the present study involved age corrections when pertinent. The mean deviation of the resulting stature estimate from the mean stature of each sample is given in table 16. It may be seen that for White males the present equations overestimate only slightly the statures of the French, Finnish, and German samples, and that these estimates deviate from the means less than do the estimates derived for the present sample according to the equations of Pearson, Telkkä and Breitinger. This difference is due mainly to the fact that the present equations provide for age differences among the groups. For the White females, the ages are more nearly comparable and thus the

TABLE 16

Mean deviation (cm) of stature estimates (based on formulae of present study and of other investigators involving femur) from the observed mean statures[1] of selected series according to race and sex

SOURCE OF OBSERVED MEAN STATURES	MEAN DEVIATION OBTAINED FROM FORMULAE BASED ON FEMUR_m						
	Present study	Pearson	Telkkä	Breitinger	Dupertuis and Hadden	Present study	Dupertuis and Hadden
	White male					Negro male	
Whites males							
Present study (American)		— 3.75	— 2.76	— 1.87	+ 3.16	— 3.82	+ 1.20
Pearson (French)	+ 0.64		+ 0.26	+ 2.55	+ 6.25	— 2.54	+ 3.10
Telkkä (Finnish)	+ 1.51	— 0.59		+ 1.72	+ 5.88	— 1.83	+ 3.14
Breitinger (German)	+ 1.03	— 2.13	— 1.44		+ 4.50	— 2.58	+ 2.00
Dupertuis and Hadden (American)	— 4.65	— 6.43	— 5.88	— 4.08		— 7.97	— 2.80
Negro males							
Present study (American)	+ 4.34	+ 0.07	+ 1.29	+ 1.71	+ 7.22		+ 5.69
Dupertuis and Hadden (American)	— 1.24	— 5.20	— 4.12	— 3.41	+ 5.06	— 5.18	
	White female					Negro female	
White females							
Present study (American)		— 1.78	— 1.30		+ 4.45	— 2.49	+ 3.37
Pearson (French)	+ 1.14		+ 0.81		+ 5.45	— 0.96	+ 4.00
Telkkä (Finnish)	+ 1.28	— 0.69			+ 5.50	— 1.00	+ 3.80
Dupertuis and Hadden (American)	— 3.55	— 5.96	— 5.37			— 5.91	— 1.21
Negro females							
Present study (American)	+ 2.65	— 0.53	— 0.16		+ 6.98		+ 5.03
Dupertuis and Hadden (American)	— 1.58	— 5.71	— 5.37		+ 0.89	— 4.27	

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[1] The observed mean stature was converted to living stature according to the directions of each investigator. Pearson presented formulae for living stature; for the present study and for Breitinger's study statures had been measured on the living; for Telkkä's series 2 cm were subtracted from the mean cadaver stature; and for Dupertuis and Hadden's series the cadaver statures were left unchanged since these authors have considered them to be equivalent to living statures.

equations of Telkkä and Pearson underestimate the mean of the present sample approximately to the same extent that the present equations overestimate the means of these samples. The equations of Dupertuis and Hadden overestimate considerably the means for every group. Their equations for Negroes even overestimate the mean statures of the White groups of the present study. It is well known that Negroes have shorter statures relative to length of femur than do Whites. Thus, it is evident that the cadaver statures as measured by Todd are greater than living statures, and that a correction is needed for these equations in this regard.

Comparison of the differences between long bone lengths and statures associated with race and sex. Many different statistics have been utilized in attempts to determine the type of variation in lengths of long bones and stature and in the relationship between these measurements associated with race and sex. The means and standard deviations of measurements obtained from various samples can be compared for significant differences. However, it is necessary that other factors, such as age, socio-economic status, period of birth, etc., be carefully controlled. The subjects constituting the Terry Collection are quite comparable with regard to these factors and a comparison for differences between these Whites and Negroes and males and females should be valid.

An examination of table 5 reveals that the Negro males and females have significantly longer bones on the average than have the corresponding sexes of the White race. (The only exception to this is the humerus of the female which does not differ significantly in length between the two races.) Also, the Negro males are significantly taller than the White males, whereas the females of the two races have an approximately equivalent stature. It may be noted in passing that in the military personnel group, the Negro male is significantly shorter on the average than the White male. Recent secular trends in stature may partially account for these apparently contradictory findings. Such findings further illustrate the necessity of defining carefully the populations from which samples are drawn for comparison. The Negro males show greater variability in every measurement than do the White males or the females of either race. The differences are statistically significant for most measurements. The White and Negro females do not differ significantly in variability nor do the White males and females.

A more interesting type of comparison between the races and sexes is that of the relative length of limb segments to each other and to stature. To this end, the ratios of average lengths in different groups have often been compared. However, such ratios can present misleading results when groups with different general size factors are compared. For example, Hrdlička ('47) indicates little or no sex difference for either the White or Negro race in the ratio of length of femur to stature, a result which was substantiated by Dupertuis and Hadden. However, when the equations for estimation of stature of Dupertuis and Hadden and of Pearson are applied it is seen that for a given length of femur the male is taller than the female.

It would appear that more meaningful questions to be answered are whether or not for a given length of one variable the groups to be compared differ in regard to other variables; and, throughout what range of measurements such differences hold true. Thus, it might be asked which sex or which race is taller when individuals with the same length of tibia or femur are compared. To answer this question the linear regression equation is admirably suited since it represents the rectified average measurement in the dependent variable for any given value of the independent variable, throughout the range of measurements. A great number of such comparisons could, of course, be made since the samples may be matched for any one of the variables studied. In order to limit the number of comparisons the length of femur$_m$ has been chosen arbitrarily as reference.

Figure 4 depicts graphically the differences in stature and in the lengths of radius, humerus, and tibia among Whites and Negroes of both sexes of the Terry Collection matched for the length of femur$_m$. It is evident that the males of each race are taller than the females for a given length of femur, and that the Whites are taller than the Negroes. However, except for relatively short statures the White females are taller for a given length of femur than are the Negro males. Likewise, for the humerus, radius and tibia, the males have the longer bones relative to the length of femur, throughout the range of measurements. The Negro also has a longer tibia and radius relative to the femur than the White but the humerus of the Negro is longer than that of the White of corresponding sex only for those individuals with short femurs. These findings substantiate the conclusion generally reached that Negroes have longer forearms and legs relative to the more proximal segments of the limbs (arms and thighs) than do White individuals, and that, in general, Negroes have longer limb bones

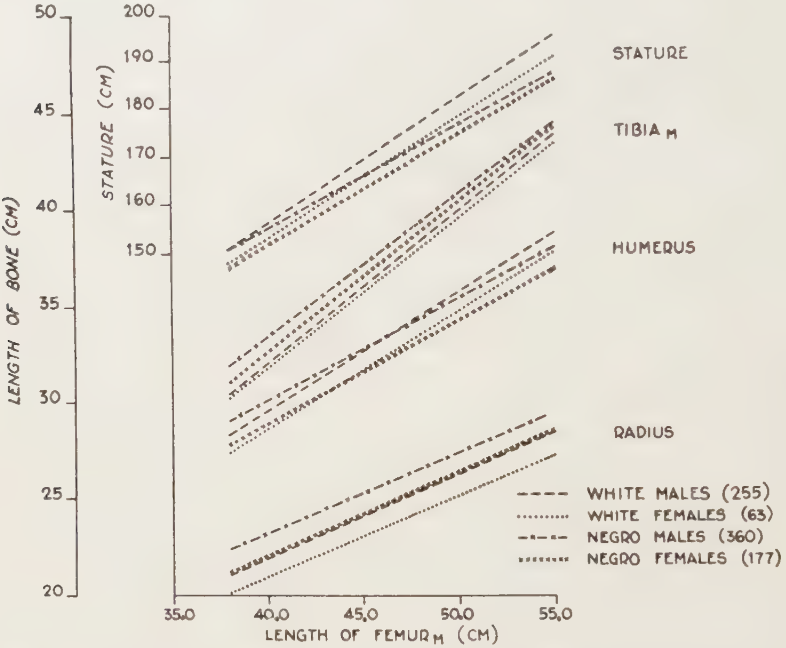

Fig. 4 Comparison of statures and lengths of long bones of Negroes and Whites of both sexes of the Terry Collection, matched for the length of the femur$_m$.

relative to their stature than do Whites. It is evident from figure 4 also that it is necessary for the sake of obtaining the most accurate estimates of stature to have different equations for each of the two sexes and for each of the two races.

A ''general'' equation or an average of the equations derived from different racial groups would necessarily result in poorer estimates of stature for any particular group or individual to which it is applied than would an equation derived from a similar group. However, if it is desired to estimate the *mean* stature of a mixed group for which the race and sex of each individual is indeterminate but for which there is *a priori* knowledge of the percentage frequency of the racial and sexual components, the most accurate result would be obtained by weighting the estimates derived from each equation according to the relative frequency of the races and/or sexes involved. And, if the stature of a single individual from such a mixed group were desired, the equation most likely to give the most accurate estimate is that pertaining to the race and sex most frequently represented in the group.

SUMMARY

The American Graves Registration Service has obligations which have stimulated interest in improvement of methods for identification of skeletal remains. Coincidentally, the ideal combination of data for the determination of formulae for estimation of stature from long bone lengths became available. These data are from American White and Negro military personnel and comprise measurements of stature during life and measurements of long bones of the free limbs after death. The Terry Anatomical Collection has been introduced into this study in order that formulae from a very different source might be provided; that these two sets of formulae, after adjustment for differences in age and in measurements of living and cadaver stature, might be tested against each other; and, that formulae for females of both races might be evolved.

Only subjects who were at least 18 years of age when stature was measured have afforded data for the equations of stature estimation. All 6 long bones were measured for maximum length; in addition, the bicondylar length of the femur and the length between the articulating surfaces of the tibia were taken. The average length of right and left bones of any given pair was utilized in the statistics because of the greater reliability of an average. Furthermore, the differences in length between the bones of the two sides are small and when the bone of only one side is available an adjustment in an equation based on the average is not necessary.

Regression equations for estimation of stature from the length of each long bone and from the lengths of multiple bones were determined for each group of subjects available from the two sources. The single bone equations are almost identical for the two lengths of femur and for the two lengths of tibia; thus only the maximum length of each bone was utilized in the multiple bone equations. Intercorrelations among the lengths of the 6 long bones are very high, particularly between radius and ulna and between tibia and fibula, so the ulna and fibula were omitted in the multiple bone equations. In both single and multiple equations the bones of the lower limb result in estimations of stature with a smaller standard error than do the bones of the upper limb.

Equations for estimation of long bone lengths (humerus, radius, ulna, tibia, fibula) from the femur are presented for Whites and Negroes of both sexes.

The increase in cadaver stature (measured according to the method of Terry) over that of living stature is estimated to be 2.5 cm. When this correction is made and loss of stature from ageing is taken into account, the equations for estimation of stature of males based on data from the Terry Collection and from the military personnel are shown to be in substantial agreement. It seemed reasonable to assume that equations based on females of the Terry Collection, with corresponding adjustments are likewise applicable to the American population of White and Negro females.

Thus, equations (determined from both single and multiple bones) for estimation of living stature of American Whites and

Negroes of both sexes are presented. These equations are applicable to maximum lengths of long bones which are dry and without cartilage. The resultant estimates are of maximum living stature and can be reduced by the amount of 0.06 (age in years — 30) cm to cover the effects of ageing. A test of the equations for White males by application to a different sample of American White military personnel gives results well within the expected range of accuracy. Comparison of statures estimated for this new sample according to equations (involving femur and humerus) developed in this study with those of other investigators demonstrates that the present formulae give the most accurate estimates of stature. Another comparison involving the application of each investigator's equation (based on the femur) to every other sample of like sex demonstrates the advantage of the age factor in the equation and also the need for an adjustment when cadaver stature (as measured by Todd) is utilized as a measurement of living stature.

The Negroes of both sexes have significantly longer bones of the free limbs than do the White groups; the Negroes also have longer forearm and leg bones relative to the arm and thigh bones than do the Whites; and, in general the Negroes have longer bones of the limbs relative to their stature. These comparisons, pointed toward the relationship of the variables, indicate the necessity of independent equations for estimation of stature for each sex of the White and Negro races.

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BRIEF COMMUNICATIONS

JINDŘICH MATIEGKA AND THE DEVELOPMENT OF CZECH PHYSICAL ANTHROPOLOGY

BOŽO ŠKERLJ AND JOSEF BROŽEK
Institute of Anthropology, University of Ljubljana, and Laboratory of Physiological Hygiene, University of Minnesota

Thin and discontinuous roots of Czech physical anthropology go back to 1850 when Jan Evangelista Purkyně, well known as a physiologist, assembled in Prague a collection of prehistoric as well as more recent skulls. A study on "Human skulls in general and the Slavic skulls in particular" was carried out by his assistant, Eduard Grégr, and published in an obscure Czech journal in 1858. From 1891 Lubor Niederle, the first *docent* of prehistorical archeology and anthropology at the Czech University, concentrated his efforts on the former field and it became the opportunity and life-long task of Jindřich Matiegka to develop the field of physical anthropology as an independent discipline concerned with man as a part of the living nature. His scientific publications cover a span of half a century from 1891, the date of his *Crania bohemica,* to the description of the rich ossuary at the town of Mělník, published in 1941. This "royal dowry town," situated some 20 miles north of Prague, furnished Matiegka not only with anthropological material but gave him also a stimulating companion and untiring helper — his wife — as well as his collaborator, immediate successor and biographer, Jiří Malý (*Jindřich Matiegka,* Czech Academy of Sciences and Arts, Prague, 1949).

Matiegka was born on March 31, 1862, at Benešov (some 40 miles south of Prague) and died at Mělník in 1941. He got his M.D. in Prague (1887) and for several years practiced medicine, mainly at Mělník and in Prague. It was in Mělník that he developed interest in anthropology. He became unpaid *docent* of anthropology and demography in 1897 but only from 1908, when appointed associate professor, could he apply his energy and organizational ability wholly to scientific work. Even then he was assigned only one room in the Zoological Institute, and he had no assistants. It was not until 1918, the year in which an independent Czechoslovakia was created, that he became — in his 56th year — full professor. In 1924 he was able to move into one of the largest institutes of physical anthropology in Europe. He had a full share of academic honors. For the year 1921–22 he was elected Dean of the Faculty of Natural Sciences and for 1929–30 the Rector Magnificus of Charles University. In 1932 he became professor emeritus and retired to Mělník where he worked with full energy until his death.

The bibliography of Matiegka's publications consists of 280-odd items, not to mention several hundred short contributions and reviews, published mostly in the journal *Anthropologie* (Prague), edited by Matiegka from 1923 to 1941. It is symbolic that with Matiegka's death, during the Nazi occupation of the "Protectorate" of Bohemia and Moravia, *Anthropologie* also perished. After World War II the money set aside for the support of the journal was "frozen," the co-editor, Malý, died in 1949, and the journal did not resume publication.

Matiegka's professional interests covered a broad field. In 1897 he published his pioneering studies on the development and health of the Prague youth and on the menarche of Czech girls; this concern remained alive throughout his life. Another field of Matiegka's activities was the reconstruction and evaluation of the skeletal remains of historical Czech personalities, such as St. Wenceslas, Czech kings and their wives (1932), Hussite warrior Žižka, Comenius, etc. A third important topic of his research was fossil hominids, especially those found at the renowned Pleistocene site of Předmostí *(Homo-předmostensis.* I. *Crania,* 1934; II. *Other skeletal remains,* 1938, Prague).

From the early beginnings of his scientific career Matiegka was also interested in demography. For years he served as a medical officer in the Health Department of the Kingdom of Bohemia. This provided material and stimulus for demographical studies. It may be noted that the university department carried the title "Institute of Anthropology and Demography." His first *docent,* František Netušil, a young demographer attached to the State Statistical Department, died in 1927 and the division of Demography remained undeveloped. Interestingly enough, one of Matiegka's last papers, "Vital statistics of the members of the Czech Academy of Sciences and Arts in Prague" (1941), was a demographic study.

Matiegka published most of his papers (and all books) in Czech, frequently with comprehensive summaries in other languages; but some studies appeared also in English, French, and German. He contributed several chapters to the volume entitled "L'égalité des races européennes et les moyens de les améliorer,'' published by the Czech Academy of Sciences and Arts (in Czech, 1934) and also by A. A. M. Stols (in French, 1935, Bruxelles). This was a prompt, courageous, and positive reaction against the Nazi racism. In 1921 he published in this Journal a stimulating paper dealing with partitioning of the human body in terms of its gross morphological components (muscles, bones, fat), a proof of Matiegka's modern viewpoint in physical anthropology of the living man.

It is impossible to cite all of Matiegka's scientific publications. However, at least two books must be mentioned. In 1927 he published *The Somatology of School Children,* a unique, comprehensive source book in this field. In 1935 appeared his profound *Philosophy* of *Somatic Anthropology.* This is the work of a mature scholar, written after a lifetime of research in many fields of anthropology.

Matiegka sometimes preferred to speak of ''somatic'' anthropology rather than physical anthropology. This terminology appears appropriate, at least insofar as anthropology is concerned with the living human organism. The term ''physical anthropology'' tended to be identified in the past with the narrow osteometric pursuits and abstract concern with body measurements without an attempt to get at the individuality of the human organism and its relationship to environmental factors, health, and the mode of life.

In addition to his intensive original investigations and field studies, Matiegka served as the editor of the journal *Anthropologie,* the *Anthropological Library* (published by the Anthropological Institute), and of the monographic series *Anthropologica* (published by the Czech Academy of Sciences and Arts). Also, he was creator of the *Hrdlička Museum of Man,* in Prague, one of the richest and best institutions of this kind in Central Europe. All these activities received an important subsidy from a special fund created by Aleš Hrdlička, the well-known American anthropologist, in memory of his wife, Marie.

Hrdlička had a profound and positive influence on Matiegka and his school. Strangely enough, in one respect Hrdlička's impact was deleterious, almost disastrous. It was he, an M.D., who obstinately held the view that the physical anthropologist must have a medical degree. Matiegka, who also came to anthropology through medicine, shared this view. Thus V. Suk, who later became professor of anthropology at the Masaryk University in Brno, Moravia, had to get an M.D. in order to be accepted into the fold although he had received his Ph.D. under the eminent anthropologist R. Martin (then in Zürich). Similarly, Malý in Prague had to study medicine as propedeutics to physical anthropology. Even if one agrees that knowledge of human anatomy is of great importance and that medical competence is vital for some anthropological problems, nevertheless the demand for an M.D. as a prerequisite for physical anthropology is excessive and wasteful. Some 50 doctor's degrees (D.Sc.) in anthropology were granted by the department between the two World Wars; but this degree was not enough to pursue academic career in this field.

In Czechoslovakia, physical anthropology was offered in the Faculty (College) of Natural Sciences. The students had little or no contact with ethnology, archeology, and other disciplines concerned with man as a social entity, as these lectures were offered in the Faculty of Philosophy. Such a separation appears undesirable and artificial. At present, familiarity with other natural sciences (such as geology and paleontology) and, perhaps more importantly, with the social sciences, is more crucial for a creative approach to anthropological research than the training in clinical medicine. It should be recognized, on the other hand, that Matiegka's direct contact with medical sciences contributed notably to the strengthening of the quantitative point of view in Czech anatomy where, under Borovanský in Prague, several excellent studies were carried out (e.g., on age changes in the length of the vertebral column, quantitative sexual differences in the human skull and pelvis, and development of ossification centers in boys from birth to 19 years).

Matiegka was a stimulating teacher, with a warm, friendly relationship to his students; he almost always found a way to solve their technical or personal problems. He was intensely interested in his work when he used to show to one of us (B.Š.) the steadily growing collections, his blue eyes would reflect the joy he had experienced in obtaining every new item for the Museum or for the library of the Institute.

In the context of this note on the development of anthropology in Czechoslovakia it may be appropriate to mention the late Professor J. Malý, born in 1898 at Mělník. He decided in his high school *(gymnasium)* years to become a physical anthropologist and after he received his M.D., Malý became Matiegka's first assistant. As a young *docent* he joined Hrdlička in a field study in Alaska. On Matiegka's retirement in 1932 Malý became the chairman of the Prague Anthropological Institute. After the Czech universities were closed by the Nazis (November 17, 1939), Malý concerned himself, as a legal consultant, with cases of doubtful paternity. Following the war he again took charge of the Institute and the Hrdlička Museum. In July, 1949, he suddenly died of a heart attack in the midst of a university examination, without a successor of professional rank primarily because of the strange non-recognition of the department's own doctor degrees.

With the passage of Matiegka and Malý, the center of the Czech physical anthropology shifted to Brno where V. Suk for some 20 years had the chair of Anthropology in the Faculty of Natural Sciences of the Masaryk University. He was in a certain sense a modernist in the Czechoslovak anthropology, interested in physiological and medico-anthropological problems. When after the war the Prague journal *Anthropologie* did not start again, Suk was able to create an Anthropological Society in Brno and publish the *Bulletin (Zprávy)*, covering the whole field of anthropological sciences in a comprehensive meaning of the term. Unfortunately, Professor Suk reached the retirement age about three years ago.

It came to our attention recently that Dr. Vojtěch Fetter, Malý's former assistant, was put in charge of the Department of Anthropology in Prague.

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WISHFUL THINKING IN THE RECONSTRUCTION OF SKULLS

T. D. STEWART
U. S. National Museum

ONE FIGURE

Recently I received from a correspondent in the Middle West the interesting skull with lower jaw shown in figure 1.[1] He asked my opinion thereof, offering at the same time his opinion that it looked like another Piltdown man, and adding the naïve explanation that the lower jaw had been supplied. The source of the skull was not stated. Likewise, no further information was provided regarding the lower jaw. But from other evidence it appears that this correspondent has more than a passing interest in early man.

In my opinion, as yet unconfirmed, the skull itself is recent Eskimo, or Pre-Aleut. All the features, but especially the pinched nasal bones and thickened tympanic plates, can be matched in collections from Alaska. The upper teeth are not original; they are made of plaster. However, the exact identification of this skull is not the important point; the important point is that it is recent and without the lower jaw would not be considered primitive, much less ancient.

The lower jaw is quite a different thing; not only has the whole of it been carefully and ingeniously constructed from plaster to fit the upper jaw, but it has been made to look primitive. Judging from its form and from the fact that my correspondent is familiar with the Piltdown jaw, it seems likely that the latter specimen served as the model.

Although the construction involved is not difficult, and it is a somewhat obvious thing to do, I am not aware that a professional anthropologist has ever attempted, or ever felt it necessary, to show how the appearance of a modern skull would be altered by adapting to it a primitive type of lower jaw. Nevertheless, the result, as figure 1 shows, is rather surprising, inasmuch as the eye promptly detects the absence of chin and the mind jumps to the conclusion that here is ancient man. The broken nasal bones and the vault elongation resulting from premature occlusion of the sagittal suture tend, of course, to heighten this effect. Obviously, then, this is another example of how misleading morphology can be by itself. This alone perhaps makes the case worth reporting.

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[1] The author exhibited this specimen at the annual dinner of the Association at the New York Academy of Sciences, March 22, 1952.

It seems to me that this case is useful from another point of view. Even though I do not know what led my correspondent to make this reconstruction, his motivation could not have been much different from that which, for example, led Montagu (this Journal, '51, n.s., *9:* 5–14) to place the modern-looking Wallbrook frontal on the more primitive-looking (and frontal-lacking) Swanscombe skull. Such demonstrations by themselves usually are fruitless exercises and prove little more than that the demonstrators have indulged in wishful thinking. In the case of Swanscombe we cannot yet estimate with any degree of accuracy the chances that the missing frontal bone had reduced supraorbital ridges. It is the same sort of ignorance that bothers us about the relationship of the Piltdown skull and jaw. Such problems cannot be solved by morphological manipulations. However, we can be sure that the chances are infinitely remote for a recent Eskimo to have a Piltdown-like jaw.

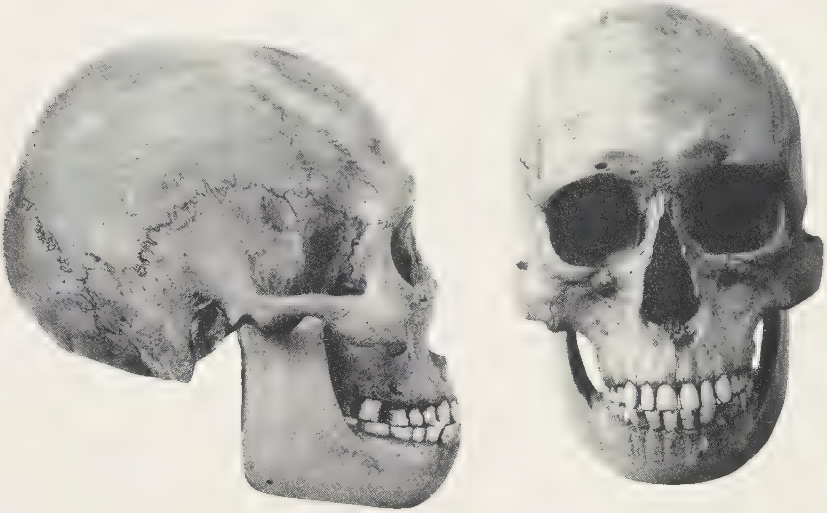

Figure 1

The Preparation of Photographic Prints for Medical Publication.— By Stanley J. McComb. Charles C Thomas, Springfield (American Lecture Series no. 90. A Monograph in American Lectures in Medical Photography, R. P. Creer ed.), 69 pp., 21 figs. 1950. $1.80.

Although books on photography are hardly rare items, few volumes are devoted to problems in medical photography, and fewer still discuss the technique of preparing photographs for publication. This little volume does both. It discusses problems of lighting, contrast and the like. It is concerned with the preparation of the print, and editorial practice as well. Physical anthropologists and medical workers face similar photographic problems, and this book in the ''American Lecture'' series should benefit both.

The Adolescent Period. A Graphic Atlas.— By Frank K. Shuttleworth. 453 illustrations. Monographs of the Society for Research in Child Development. Vol. 14, no. 1, Serial no. 49, Child Development Publications, Evanston. 1950.

This book is a collection of graphs on almost every possible topic pertaining to the adolescent period, and some topics that relate to adolescence only indirectly. There is information on menarche, the circumference of the flaccid penis, the percentage of boys who buy their own clothes, the median age at marriage, proportions of married, divorced and widowed women at various ages, and much more. A good deal of the material has been redrawn, and the graphs are simple, and ideally suited for lantern slides. A few of the graphs probably include too much unrelated material, and of course the graphic method of presentation gives equal prominence to data derived from large series, and data derived from small series. However the ''Adolescent Period'' is really an illustrated bibliography, a time saver, and a substitute for a cross-reference card file.

REVIEWS

EVOLUTION EMERGING, A Survey of Changing Patterns from Primeval Life to Man. By William King Gregory. Volume I: Text; Volume II: Illustrations. New York, 1951.

This stupendous work is the fitting climax of the career of one of our greatest American scientists. Dr. Gregory is primarily a paleontologist, but also a first-rate comparative anatomist and, for many years, a leading student among physical anthropologists of the United States. Indeed, he was one of the early presidents of the American Association of Physical Anthropologists and more recently has been a recipient of the Viking Fund Medal for distinguished service to physical anthropology by vote of the Association. He has, of course been accorded many other well-deserved honors, but this review and appreciation naturally will stress the importance of Gregory and of *Evolution Emerging* to physical anthropologists.

In the first place, the well-equipped teacher and practitioner of physical anthropology has to know a good bit about the dentition — not only as pertaining to contemporary man and the other Primates, but also in connection with fossil man, fossil Primates, and from there right back to the beginning of teeth. The best source for such information, for the last 35 years, according to the opinion of this reviewer, has been the many works of Dr. Gregory. Others have written voluminously and capably on this subject, but none is so dependable an authority. It is a fearsome and intricate specialty and Gregory is master of it. The physical anthropologist also must know something about the evolution of the bones of the skull and of the postcranial skeleton and here, again, Dr. Gregory is an infallible guide and instructor. And, supposing that our physical anthropologist is interested in the philosophy of evolution, he can go elsewhere if he likes, but, in my opinion, he will do well to stick with W. K. G. Dr. Gregory is a versatile and almost Protean scientific writer. He can overwhelm the reader with paragraph after paragraph of technical detail and then summarize and clarify the whole business in a few lucid sentences, thus terminating the bewilderment and semantic frustration of the struggling student and allowing him to close the book and go away with the salient facts and basic principles embedded in his mind.

Of course, the writer on paleontology has the initial terrible burden to bear that has been heaped upon him by colleagues and predecessors in the shape of the polysyllabic Greek and Latin compounds invented to designate categories of animals all the way down to subspecies and varieties, as well as the parts of their bodies and their life processes. It is impossible for the paleontologist to avoid the use of this awful nomenclature by circumlocutions and, when he covers the whole vast range of animal evolution, he necessarily peppers his text with sesquipedalian verbiage which enormously dismays the reader, especially if the latter "has little Latin and less Greek." Gregory has the pleasant habit of making these nauseous terminological doses more palatable by coating them with descriptive phrases often drawn from the vernacular. For example: ANOTHER BAD LOT (TRACHINOIDEI): DOUR WEAVERS, LURKING STARGAZERS, OGLING DRAGONETS, AND DARTING SAND-LANCES.

I doubt that any anthropologist, or perhaps even any paleontologist is likely to sit down and read *Evolution Emerging* from end to end. It is more suitable for frequent dips than for prolonged immersion. It is like the Bible, in that one can read a chapter here and a chapter there with great pleasure and profit, and it is like an encyclopedia in that one can find in it (if one is an anthropologist, or a comparative anatomist, or a paleontologist) succinct and authoritative information on nearly any subject (within the vast scope of the work) on which one wishes to "bone up." For example, this work came into the hands of the reviewer just when he was beginning to give for the nth time an elementary lecture course on human evolution. It was most helpful to be able to turn to the pages in which Gregory records his mature opinion upon the phyletic position of e.g. *Proconsul* and a score of other subjects. In addition to the information which one from time to time may need in his business, it is pleasant to read for relaxation such a diverting little essay as THE PALIMPSEST THEORY OF THE ORIGIN OF THE MONOTREMES, or ALL ABOUT THE AQUATIC ADAPTATIONS OF THE WHALEBONE WHALES. In other words, Professor Gregory's colossal opus is not really grim reading at all, although it would perhaps be a little more accurate to say that not all of it is grim reading. I find Gregory most amusing when he breaks into Lucretian verse about the theory of evolution:

> "Deep in the womb the living globule waits
> The bliss of union with her spermal mate."

The reviewer has, from time to time, yielded to the temptation to leaven the sour lump of his textual writings with verse, but he has usually put the stuff in small print in the footnotes and ascribed it to some fictitious author, because he is ashamed of it. But Professor

Gregory's 19 stanza "Argument: Evolution Emerging" is there in the Introduction, under the chapter caption "The Cosmic Cinema." And, indeed, Professor Gregory has no reason for being ashamed of his verse; it is respectable and serious stuff. Anyone who can work "anisomeres" into blank verse commands my admiration.

"But equal parts to parts unequal changed
Are, on the contrary, called 'anisomeres.'
As when the rows of teeth almost alike
Gave rise in crocodilians to festoons."

With this may be compared the following despicable doggerel:

"The shark has teeth set like a saw
In serried ranks within each jaw.
When one set gets the worse for wear,
He simply pushes in a spare.
Equipped with these successive dentures
He has carnivorous adventures,
And since he cannot bear the sight of you
He turns on his back to take a bite of you."

Abandoning these envious comparisons, the reviewer wishes to state his unqualified admiration of this huge masterpiece of Dr. Gregory and his own intention to keep it on his desk and to "read, mark, learn and inwardly digest" its invaluable contents as long as he is able to do any of these four things. Every physical anthropologist who can find $20 ought to purchase *Evolution Emerging,* even if he has to sell his wife's mink-dyed muskrat coat to get it.

The captious reader of this review will perhaps remark to himself or to others: "The review is not at all a satisfactory summary of the *Evolution Emerging.* It gives no adequate account of the ground covered in the work. It does not even call attention to the regrettable fact that on p. 319, section 13, the *m* in *larolimicolae* is broken in the text." This is all too true. Dr. Gregory's compendium and digest of organic evolution is far beyond our poor power to adulate or to detract. We can only state as loudly as possible our conviction that this is a great work written by a great man.

E. A. Hooton
Harvard University

FUNCTIONAL ENDOCRINOLOGY FROM BIRTH THROUGH ADOLESCENCE. By Nathan B. Talbot, Edna H. Sobel, Janet W. McArthur and John D. Crawford. xxx + 638 pp., 212 figs., 59 tables. Published for the Commonwealth Fund by the Harvard University Press, Cambridge, Mass., 1952. $10.00.

This is an admirable book, the product of the joint labors of a team of workers at the Harvard Medical School. It combines the virtues of a first-rate textbook with the report of original investigations. Designing it for readability as well as assisting the reader to do some independent thinking, the authors have made a special effort to be as helpful as possible to the reader having no previous familiarity with the subject of endocrinology. Written for practitioners, students, and investigators of medicine and surgery who seek practical information concerning (a) the actions of endocrine systems in health and ordinary disease and (b) the management of endocrinopathies as they occur in young people, this book will be indispensable to all physical anthropologists in any way interested in growth and development.

Ably translating into modern clinical terms the theme so beautifully developed by Walter B. Cannon in his classic writings on *The Wisdom of the Body*, the authors have produced a book which is likely to become a classic in its field as well as a model for others. Each gland is treated in a systematic manner, from the ground up, as it were: chemistry, hormone actions, relations to other glands, effects of therapy, physiologic variations, methods of appraising glandular status, biochemical factors, morphologic and physiologic changes, clinical considerations, case histories, diagnosis, differential diagnosis, treatment, prognosis, and much else, as for example, in reference to the menstrual cycle: "To visualize the phenomenon of menstruation in correct perspective, it is important to appreciate the fact that periodic uterine bleeding is of comparatively recent evolutionary origin. A process peculiar to primates, menstruation represents a by-product of the anatomical changes imposed upon mammals as viviparity became an evolutionary goal. The longer the retention of the embryo in the uterus, the more complex became the endometrial preparation for the implantation and nutrition of the fertilized ovum.

If, during a given cycle, fertilization did not occur, this succulent and elaborately vascularized lining was 'aborted' and the entire structure reconstituted during the succeeding cycle'' (p. 309). Each chapter is followed by a list of general and specific references. The figures, photographs, and other illustrative material, though printed on non-finished paper, have come out remarkably well, and the 59 tables and additional nomographs for estimating surface-area from body-weight and height are very helpful. There is a most useful appendix which gives the reader the references to the methods followed in the tests throughout the book. There is a full index.

If there is one criticism, or to put it more accurately, sigh that I heaved, it is with regret for the chapter the authors considered writing but decided not to write because, according to them, there was not enough information available to justify it. This chapter, as they say, might have been called Neuro-endocrine or Psycho-neuro-endocrine relations. More shortly, it might have been called the Neuro-Humoral System. It is, no doubt, because the authors are so familiar with the field that they realize the difficulties involved in writing such a chapter. It could have been wished that they had written it in spite of the difficulties, for difficulties are made to be overcome, and one of the best ways of overcoming them is to do precisely what the authors have done in this volume in which they have, for the first time, filled some of the gaps in our endocrinological knowledge, as a result of reviewing the literature and becoming acquainted with the lacunae which existed. It is devoutly to be hoped that in a future edition of this exemplary work they may be persuaded to add a chapter on the Neuro-Humoral System.

M. F. Ashley Montagu
Rutgers University

ESTIMATION OF BODY FAT.— One of the fundamental characteristics of man's state of nutrition is the composition of his body. Grossly, at least, this means the proportion of his body-weight accounted for by the basic components of blood (subdivided into plasma and cells), interstitial fluid, bone, fat, and "active tissues" (principally muscles, glands, and nerves). Body fat shows the most striking variations in states of nutrition varying along the emaciation-obesity continuum. The provision of improved methods for a quantitative estimation of the relative fatness is one of the pressing tasks of nutritional science; it is essential both for the evaluation of calorie nutrition and for the establishment of valid estimates of calorie requirements. . . .

In living man the percentage of the body represented by fat, estimated on the basis of specific gravity of the body, appears to be the best single criterion for characterizing the individual's leanness-fatness. The skinfolds may be regarded as the next best index. . . .

In the present study the validation of the more indirect or partial indicators of fatness was carried out in terms of correlations with the specific gravity. The chest skinfolds showed the highest correlations with specific gravity in both age groups ($r = -0.857$ for the younger men, $r = -0.682$ for the older men). The relative weight showed a surprisingly high correlation (-0.783 and -0.633 respectively). However, it should be noted that this relationship holds only within a limited age range. . . .

A valid empirical selection of the locations of a small number of skinfolds out of an infinite number of possible points still remains to be carried out. In addition to fulfilling such a requirement as accessibility, the points at which the skinfolds are to be measured must have a definite location, facilitating repeatability of the measurements. The selected measurements should correlate highly with the total body fat, while having relatively low correlations with each other. . . .

The amount of body fat exhibits larger differences between normal individuals than perhaps any other body constituent. . . .

In the present study the "storage" fat, estimated from the specific gravity, varied in the younger men from zero to 32.7% of the body-weight, in older men from 1.4 to 34.2%; the other fatness criteria also exhibited large individual differences. Part of these differences may be explained as "error" of measurement, including such items as the failure to measure residual air in each individual, but the major variation is undoubtedly simply interindividual difference. J. Božek and A. Keys. The evaluation of leanness-fatness in man: Norms and interrelationships. Brit. J. Nutrit., vol. 4, no. 2, 1951, pp. 194–206.

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